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## CUNEIFORM TEXTS AND HELLENISTIC CHRONOLOGY

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**B**ABYLONIAN sources have yet to make their most important contributions to our knowledge of the Hellenistic period. Now and then they add to the narrative history. When compared with the more numerous documents from the Chaldaean and Achaemenid periods, Seleucid documents show important changes in economic and social conditions;<sup>1</sup> important changes may also be seen in a native religion which at first sight appears static. Equally significant changes appear in neighbor religions influenced at this time by Babylonian.<sup>2</sup> One of the most wished-for of the unwritten chapters in the history of science is that which marks the last bloom of Babylonian astronomy about the beginning of the second century before our era.<sup>3</sup> Only the value of Babylonian tablets for the tangled Hellenistic chronology has been adequately recognized, and lists presenting their data have been repeatedly published.<sup>4</sup>

Not only must these lists be brought up to date; what is far more

<sup>1</sup> To be shown in a forthcoming study by Dr. Waldo Dubberstein.

<sup>2</sup> Cf. A. T. Olmstead, *Jour. Amer. Oriental Soc.*, LVI (1936), 242-57.

<sup>3</sup> Studies in the history of Babylonian astronomy are in preparation.

<sup>4</sup> J. Strassmaier, *Zeitschrift für Assyriologie* (hereafter cited *ZA*), VII (1892), 201 ff.; VIII (1893), 106 ff.; F. X. Kugler, *Sternkunde und Sterndienst in Babel* (cited as *SK*), II (1909), 438 ff.; *Von Moses bis Paulus* (1922) (cited as *Moses*), pp. 301 ff.; E. H. Minns, *Journal of Hellenic Studies*, XXXV (1915), 34 ff.; A. T. Clay, *Babylonian Records in the Library of J. Pierpont Morgan* (1913) (cited as *BRM*), Part II, pp. 13 f.; O. Krückmann, *Babylonische Rechts- und Verwaltungsurkunden aus der Zeit Alexanders und der Diadochen* (1931), pp. 20 ff.; E. Unger, *Babylon* (1931), pp. 318 ff.

important, they must be subjected to rigorous criticism and the conclusions deduced therefrom must be examined afresh. Impressed by the emphatic and repeated statements that certain conclusions are based on scientific calculations from astronomical tablets, perhaps also impressed by the mystery of the cuneiform writing, historians have often failed to apply the most elementary principle of historical criticism—the vital distinction between contemporary and later documents. Since they do not read cuneiform or study astronomy, their conclusions will, as a rule, be accorded the same charitable silence it is hoped will be granted the unwitting sins of the present writer; unfortunately, it is impossible to avoid criticism of the great master of Babylonian astronomy. Those who themselves have followed from afar the sure progress of the late Pater F. X. Kugler through the mazes of Babylonian astronomy must always regret that he abandoned his promised third volume of the *Sternkunde*, now lost to us forever, to tread the more alluring, but for him less safe, bypath of chronology.

We cannot too strongly insist that for safe chronology we must depend only on contemporary records. One native history has been preserved, the so-called "Diadochi Chronicle," clearly written within the generation and fitting well what is known from Greek sources.<sup>5</sup> Of formal royal inscriptions we have a single example from Antiochus I.<sup>6</sup> Of the fairly numerous administrative and business documents from the Seleucid and Parthian periods but few have been published;<sup>7</sup> their date formulas must form the backbone of our investigation. In addi-

<sup>5</sup> Sidney Smith, *Babylonian Historical Texts* (1924), pp. 124 ff.; for corrected chronology of the second half cf. W. Otto, *Sitzungsberichte der Bayerischen Akademie* (1925), p. 3; Smith, *Revue d'Assyriologie*, XXII (1925), 179 ff.

<sup>6</sup> H. Rawlinson, *Cuneiform Inscriptions of Western Asia*, V, 66; F. H. Weissbach, *Die Keilinschriften der Achämeniden* (1911), pp. 132 ff.

<sup>7</sup> Strassmaier, *ZA*, III (1888), 129 ff.; A. T. Clay, *loc. cit.*; O. Schroeder, *Kontrakte der Seleukidenzeit aus Warka* (1916) (cited Schroeder); L. Speleers, *Recueil des inscriptions de l'Asie antérieure des Musées Royaux du Cinquantenaire à Bruxelles* (1925) (cited Speleers), pp. 32 ff., 100 ff.; G. Contenau, *Contrats néo-babyloniens* (cited Contenau), Part 2 (1929); R. C. Thompson, *Catalogue of the Late Babylonian Tablets in the Bodleian Library* (1927), translation only. Scattered publications: C. F. Lehmann, *ZA*, VII (1892), 328 ff.; T. G. Pinches, *Cuneiform Texts from Babylonian Tablets in the British Museum*, Vol. IV (1898), Nos. 29d, 39c; A. T. Clay, *Legal and Commercial Transactions, Dated in the Assyrian, Neo-Babylonian, and Persian Periods* (1908), No. 129; A. Ungnad, *Vorderasiatische Schriftdenkmäler der königlichen Museen zu Berlin*, Vol. VI (1908), No. 227; J. B. Nies and C. E. Keiser, *Historical, Religious, and Economic Texts and Antiquities* (1920), Nos. 135 f.; A. Boissier, *Babyloniaca*, VIII (1924), 27 ff.

tion, we have various citations of date formulas from unpublished tablets, which are equally valuable where we are sure that they are contemporary! Unfortunately, of the most used lists—those of Strassmaier<sup>8</sup>—the earlier appears without exception to be taken from unreliable sources and the later is not free from suspicion.

Despite repeated and emphatic claims made for the higher accuracy of the astronomical dates, the exact reverse is generally true. Now and then we do find a contemporary yearly observation of astronomical phenomena intermixed with notes on prices or contemporary events, which in the so-called "Antiochus Chronicle" almost reaches the dignity of formal history.<sup>9</sup> True date formulas on astronomical tablets are equally trustworthy but somewhat rare during the Seleucid period; they are more numerous under the Parthians, but rarely tell us more than that the king was Arshaka (Arsaces), and the majority have been published only in part.

Sharply to be differentiated from the facts secured from contemporary documents are the statements given by later astronomers in their calculated tables. The great historian of antiquity, Eduard Meyer, warned against their use,<sup>10</sup> and his warning should have been better heeded. Actual test has shown that, where they had opportunity of error, they have made full use of that opportunity. The majority come from the Parthian period, long after the events they report; the smaller number, shown by calculation to come from the Seleucid period, have the most serious errors!

Of all astronomical sources employed by recent students two are unusually dangerous. The so-called "Saros Canon" is certainly not earlier than 248 B.C.—more probably it comes from the Parthian period;<sup>11</sup> the other is an even cruder eighteen-year cycle which runs to 99 B.C.;<sup>12</sup> neither represents the high astronomical achievements of the period. These are found in the ephemerides, the tables for calculations of new moons and eclipses, above all in the planet tables which mark the climax of Babylonian science; as a rule, the astronomers con-

<sup>8</sup> ZA, VII (1892), 202 ff.; VIII (1893), 106 ff.

<sup>9</sup> Smith, *Bab. Hist. Texts*, pp. 150 ff.; cf. Kugler, *Moses*, pp. 318, 338 ff.; SK, II, 442.

<sup>10</sup> *Forschungen*, II, 460.

<sup>11</sup> Strassmaier, ZA, VIII, 149 ff.; X, 64 ff.

<sup>12</sup> T. C. Pinches, *Proc. Soc. Biblical Archaeology*, VI (1884), 202 ff.; Strassmaier, ZA, VII, 197 ff.; VIII, 106 ff.; Kugler, SK, II, 364, n. 1.

fine themselves to noting on the margin the Seleucid date. When they attempt to indicate the ruler, their results are as unsatisfactory as those of their famous modern successor.

We have no published cuneiform records from Alexander the Great; those formerly so attributed come from the reign of his son of the same name.<sup>13</sup> For the reigns of Philip Arrhidaeus and Alexander II, we have the already mentioned Diadochi Chronicle. The last date of Philip is year 8, Abu 20 (August 24, 317 B.C.).<sup>14</sup> Contemporary usage then employed the first year of the boy Alexander.<sup>15</sup> After Antigonos had driven out Seleucus, documents were dated from his first to his sixth year, though only as *rab uqu* or general.<sup>16</sup>

When Seleucus recovered Babylon in 312, he gave order that the people should no longer date by the sixth year of Antigonos but by that of the young Alexander;<sup>17</sup> the first document is dated year 6, Simannu 4 (June 14, 312 B.C.).<sup>18</sup> Next year was announced as "Year VII of Alexander, son of Alexander, and Seleucus,"<sup>19</sup> and this is confirmed by an astronomical text of year 90 (222 B.C.), "Year I of Seleucus which corresponds to year VII"—the more trustworthy as our astronomer quite evidently did not know what it meant.<sup>20</sup> The last date of "Alikсандar, king of lands," is year 9, Airu 8 (May 17, 309 B.C.);<sup>21</sup> the first date of "Silluk the king" is year 8, Nisannu 3 (April 16, 304 B.C.).<sup>22</sup> Dating is henceforth by the Seleucid era, which in Babylonia began Nisannu 1 (April 3, 311 B.C.).

<sup>13</sup> Unpublished contracts dated from his reign are referred to by Smith, *Revue d'Assyriologie*, XXII (1925), 185.

<sup>14</sup> Contenau, No. 249. That the Saros Canon gives year 6 of Philip as 318 means nothing. The dates B.C. are taken from an unpublished calendar compiled with the aid of Dr. Waldo Dubberstein.

<sup>15</sup> Contrary to a widely held opinion—the dangerous result of a little knowledge—the ancient custom of the "accession year" and the consequent postdating still employed by the Achaemenids were abandoned by the Macedonians.

<sup>16</sup> Pinches, *Proc. Soc. Biblical Archaeology*, VI (1884), 202 ff.; "the number of the tablet is not stated and is unknown to me" (Smith, *op. cit.*, p. 179), but Pinches (*Nimroud Central Saloon* [1886], p. 123, No. 109) gives a tablet of Ululu (September), year five. No stress may be laid on the fact that the eighteen-year cycle gives the first year of Antigonos as 316, the Saros Canon as 317.

<sup>17</sup> Diad. Chron., rev. 2.

<sup>18</sup> Strassmaier, *ZA*, III (1888), 137, 150.

<sup>19</sup> Diad. Chron., rev. 13; cf. Smith, *Revue d'Assyriologie*, XXII (1925), 189 f.

<sup>20</sup> J. Schaumberger, *Analecta Orientalia*, VI (1933), 7; cf. below.

<sup>21</sup> Contenau, No. 248.

<sup>22</sup> Pinches, *Cuneiform Texts*, IV, 29d; year (Strassmaier, *ZA*, VII, 202) is dubious. The text (*ibid.*, III, 135 f., 148 f.) is rightly assigned to year 11 of the Seleucid era, not to the reign of a Demetrius (Krückmann, *op. cit.*, p. 21).



Years since, Kugler announced that new evidence, firmly fixed by astronomy, proved that Antigonus recaptured Babylonia in 302 while Seleucus was fighting in India, and this enforced a change of the decisive battle of Ipsus from the supposedly fixed date of 301 to 300.<sup>23</sup> At long last, the cuneiform document itself lies before us.<sup>24</sup> It contains calculated, not observed, eclipses and ephemerides of the planets; by calculation Kugler dates it to year 90 (222 B.C.), and so eighty years after the battle. It begins with computation for a lunar eclipse on Nisannu 13, no year given, continues with other eclipses which Kugler calculates for year 72 (240 B.C.), and only in the tenth line has "year 10 Si king," after which we have other computations for planet ephemerides. Two lines later we find the crucial reference, 14(?)*(kam)* (*m*)*An-ti-gu-nu-us*. First comes a numeral of uncertain reading; since the sign for "year" is missing, the possibility that it is a day of the month must be left open. The title "king" is conspicuously missing after "Antigonus." Immediately there follows the month Duzu (July), night of the eighteenth, when the calculated phenomena took place. This is absolutely the only basis for the far-reaching changes of Kugler! When, in addition, we find Kugler himself pointing out that our astronomer calculated an eclipse which never took place, we argue that, if he did not know his own proper business, we can scarcely draw assured historical conclusions from this badly muddled passage. Finally, our confidence is not increased when we find another late scribe beginning the rule of Antigonus as early as 317.<sup>25</sup>

Kugler also finds evidence for the dating of Ipsus to 300 B.C. in a statement of Malalas<sup>26</sup> that the foundation of Seleucia by Pieria took place on April 23 and of Antioch on May 22, immediately after the battle of Ipsus. Undoubtedly Malalas has preserved good material, but those who have labored repeatedly to extract the wheat from the chaff will not press too hard his details. Those who have duplicated the march from Ipsus in the days before the automobile will find a greater difficulty in the topography. We may disregard the difficul-

<sup>23</sup> SK, II, 438 f.; *Moses*, pp. 307 ff.

<sup>24</sup> Kugler, *Orientalia* (N.S.), II (1933), 105; J. Schaumberger, *loc. cit.*

<sup>25</sup> Smith (*Revue d'Assyriologie*, XXII [1925], 183 f.) notes: "According to 'the 18 year list,' Antigonus' 14th year would then be 303/2, not 302/1, as would be demanded by the equation with the 10th year of Seleucus. Owing to this discrepancy, Kugler seems to have tacitly corrected the date to '15th year of Antigonus.'" Smith then states that a renewed collation of the tablet leaves the question of reading 14 or 15 in doubt.

<sup>26</sup> viii. 199.

ties of collecting and marching armies over the plateau during the late winter, though in the wars of the Diadochi, we are told, armies regularly went into winter quarters. If we place the battle of Ipsus as early as April 1, if we allow no interval for recuperation after a hard-fought battle, we have but twenty days to march over a wind-swept plateau and through two sets of passes, one of which at least was still filled with drifted snow. These twenty days would cover a distance estimated in Murray's *Guide* as a total of about one hundred and sixty horseback-hours. The royal progress with soldiers wearied from a hard battle and with nothing more important ahead than city foundations would, according to Kugler's hypothesis, necessitate a march of eight calculated hours, say twenty-five miles, per day. There is, therefore, no reason to refuse 301 B.C. as the date of Ipsus.

Henceforth, one of our chief problems is the more exact dating of the changes in rulers, including the association of sons. Here especially it is necessary to differentiate between contemporary date formulas and marginal notations in calculated astronomical tablets of a much later time. The last certain date of Seleucus alone is year 16, Arahsamna 16 (November 24, 296 B.C.);<sup>27</sup> the first of "Siluku and Attaikusu, kings," year 20, Kislimu 20 (December 14, 292 B.C.);<sup>28</sup> the last, year 31, Kislimu 10 (December 2, 281 B.C.).<sup>29</sup>

Antiochus I associated his son Seleucus at his accession, 280 B.C.,<sup>30</sup> the last date is year 43, Addaru 20 (March 27, 268 B.C.).<sup>31</sup> Antiochus had associated another son, Antiochus, by Second Addaru (March 25-April 23, 264 B.C.).<sup>32</sup> Business documents prove the same association

<sup>27</sup> Clay, *BRM*, II, 13.

<sup>28</sup> *Ibid.*, Vol. II, No. 3.

<sup>29</sup> *Ibid.*, No. 5; Kugler (*Moses*, p. 309) quotes a tablet with only Seleucus for year 19, apparently not contemporary; Clay (*BRM*, II, 13, n. 1) "records only Seleucus for the 31st year," but Kugler's explanation, "Demselbem offenbar misstrauend hat Clay leider kein Monat- und Tagdatum angegeben," is less plausible than the assumption that the tablet is broken. Year 32, quoted from Strassmaier by C. F. Lehmann-Haupt (*Klio*, III, 526), seems to be the "Si king" of the planet ephemeris published by Kugler (*SK*, I, 84), and there restored year [1] 32; correct accordingly Kolbe, *Beiträge zur syrischen und jüdischen Geschichte* (1926), p. 13, n. 1.

<sup>30</sup> Antiochus Chron., p. 17; first business document (Schroeder, No. 23), year 33, Tashritu 10 (October 12, 279 B.C.).

<sup>31</sup> Weissbach, *loc. cit.*

<sup>32</sup> Clay, *BRM*, Vol. II, No. 11; the broken date might be 46 except for intercalation which proves year 47. Strassmaier (*ZA*, VIII, 108) begins the association a year earlier, and gives for year 47 "Anti'uksu, great king, and Anti'[uksu, his son, kings,]" but the formula "great king" is suspicious and should indicate that the astronomical tablet was written under Antiochus III who did use this formula.

of father and son for year 48, Nisannu 6, (April 28, 264 B.C.), and year 49, Kislimu 21 (December 25, 263 B.C.). The natural assumption is that the two Antiochi were associated during these years.<sup>33</sup>

Against this assumption Kugler quotes two unpublished astronomical tablets.<sup>34</sup> One whose date is lost he calculates to be from year 49; it reads "Anti'uku and Siluku, kings." The other is restored [4]9 and reads "An and Si, kings." Kugler, therefore, argues that Seleucus was dropped from association between years 43-46 (from his own data the latter should be 48), that Seleucus was restored a few months in 49, but that by the end of the year Antiochus was back. Again we find that the tablets were computed in years 95 and 96, 217/6 and 216-5 B.C., forty-six and forty-seven years later; in the following year, 50, we have correctly "Antiochus and Antiochus," though here is another ground of suspicion. The title of "great king" is given to Antiochus I, witnessed elsewhere only for year 47 in an already cited astronomical tablet, also written under Antiochus III. Current opinion assumes that Antiochus III borrowed his title of the "Great" from the Babylonian *šarru rabu*, "great king"; the title never is found in contemporary date formulas and the references just cited for Antiochus I, but dating from Antiochus III, give the only evidence.

The last sure date of Antiochus I is year 50, Addaru 18 (March 8, 261 B.C.);<sup>35</sup> the first of Antiochus II alone, year 51, Shabatu 15 (February 22, 260 B.C.).<sup>36</sup> The title "king of lands," though common down to Achaemenid times and repeated by Philip and Alexander II, is suspect when cited for year 65, though "Antiochus, king of lands," does appear once, probably of Antiochus III.<sup>37</sup> After so many unreliable astronomical texts it is a pleasure to cite one which is reliable and

<sup>33</sup> Clay, *BRM*, Vol. II, Nos. 12 f.

<sup>34</sup> *SK*, II, 439; *Moses*, pp. 313 ff. A supposed third, "year 49, An and Si kings" (Strassmaier, *ZA*, VIII, 108), is evidently Kugler's second.

<sup>35</sup> Contenau, No. 236. The "year 51, An," with sufficient space for another "An," from an unpublished astronomical tablet (Strassmaier, *ZA*, VIII, 108), naturally does not permit Kugler (*Moses*, p. 316) to claim that Antiochus was still living April 17, 261 B.C.

<sup>36</sup> Clay, *BRM*, Vol. II, No. 14; Kugler himself (*Moses*, p. 317, n. 1) admits that the astronomical tablet (Epping-Strassmaier, *ZA*, VI, 94), "year 50, Siluku king," is an error for Antiochus II. His comparison of the one certain error in a contemporary business document (Strassmaier, *ZA*, III, 137 ff., 150 ff.) is not to the point; for though once the scribe writes down the name of the recently deceased Seleucus III, in the other references in the body of the document he gives correctly Antiochus III.

<sup>37</sup> Strassmaier, *ZA*, VIII, 109; Schroeder, No. 20.

contemporary, with its reference to the death of the lady [As]tarat-niki, the queen mother Stratonice, computed to year 58, 254/253, in Saparda or Sardis; like previous contemporary astronomical tablets, it noted current events and prices, a welcome addition to our scanty economic data from the Seleucid period—had Kugler thought it worth while to cite them.<sup>38</sup>

The last date of Antiochus II is year 66, Nisannu 12 (April 15, 246 B.C.);<sup>39</sup> the first of Seleucus II, year 67, Simannu 22 (July 12, 245 B.C.).<sup>40</sup> The most important document of the reign is the lost tablet, formerly in the Ward Collection, of year 75, Addaru 8 (February 21, 236 B.C.), which mentions Seleucus, Antiochus (Hierax), and the queen mother Laodice.<sup>41</sup> On the basis of the cuneiform material it is impossible to distinguish between the reigns of Seleucus II and Seleucus III, whose last date is year 89, Simannu 24 (July 11, 223 B.C.).<sup>42</sup>

Although the future Antiochus III was satrap of Babylon under his elder brother, Seleucus III, he does not make a certain appearance until year 90, Shabatu 21 (February 17, 221 B.C.).<sup>43</sup> New light for Molon's revolt is cast by the business documents, and especially by the extracts from the liver omens copied with especial relation to the revolt. Uruk in South Babylonia was loyal to Antiochus, for we have dates year 91, Airu 4 (April 30), and Duzu 7 (July 1, 221 B.C.).<sup>44</sup>

At this point comes an example of Kugler's method, so instructive and so urgent a warning to those who have hitherto trusted his statements that it demands fuller presentation. After noting correctly that the war against the Parthians in 209 B.C. necessitated the association of his twelve-year-old son, Antiochus, Kugler continues:

Merkwürdigerweise werden aber im Schlussdatum der Inschrift II, aus dem folgenden Jahre 104 SA (=208/7 v. Chr.) [neben dem König] *aplāni*

<sup>38</sup> *Moses*, p. 318; *SK*, II, 440; Strassmaier, *Cambyses*, No. 400; cf. *SK*, I, 61 ff. for similar tablets.

<sup>39</sup> Contenau, No. 238.

<sup>40</sup> Clay, *BRM*, Vol. II, No. 17.

<sup>41</sup> Lehmann, *ZA*, VII, 330 ff.

<sup>42</sup> Clay, *BRM*, Vol. II, No. 28.

<sup>43</sup> J. Oppert and J. Menant, *Documents juridiques de l'Assyrie et de la Chaldée* (1877), pp. 313 ff. Thureau-Dangin, *Tablettes d'Uruk à l'usage des prêtres du Temple d'Anu au temps des Séleucides* (1922), No. 10, may be earlier—year 90, day 6 plus of unknown month, Antiochus king. "An king" is cited by Strassmaier (*ZA*, VIII, 109) for year 90, but Kugler (*SK*, II, 440) shows that the calculations are for year 96.

<sup>44</sup> F. Thureau-Dangin, *op. cit.*, No. 1; Contenau, No. 241; cf. Olmstead, *op. cit.*, LVI (1936), 242 ff.

*šarri*, "Söhne des Königs" erwähnt, von denen der zuletzt genannte *An-ti'-uk-su* heisst. Ist dieser Antiochos der vorgenannte Kronprinz? Schwerlich! Denn es schickte sich nicht, dass man den ältesten Sohn dem jüngeren Seleukos (der dann als erster in Frage käme) nachgesetzt hätte, zumal diesem erst viel später die Ehre der Mitregentschaft zuteil ward. Deshalb erscheint es unabweisbar, dass es sich um den jüngeren gleichnamigen Bruder, den späteren Antiochos IV. Epiphanes handelt, der 10 Jahre später (198 v. Chr.) mit seinem ältesten Bruder Antiochos auch an dem Kampf gegen das ägyptische Heer am Panion teilnahm . . . . Ob diese Mitregentschaft im Grunde nichts war als eine vorübergehende Ehrung oder ob das Gebiet, dem das Dokument (c)=SH. 504 entstammt, dem königlichen Knaben in besonderer Weise unterstellt war, während der ältere Antiochos als mitregent für das ganze Reich galt (siehe sogleich), lässt sich nicht entscheiden. Aber letzteres dünkt mir am wahrscheinlichsten.<sup>45</sup>

After this amazing information about Antiochus Epiphanes, doubtless to be utilized in future commentaries on the Book of Daniel, we turn to the transliterated text on the previous page and read: *šattu 104(kam) . . . (m)An-ti'-]uk-su aplani(pl) šarri(!)*, "Year 104 [ . . . Antiochus, sons (of?) the king." This is a true colophon of a contemporary astronomical tablet, but what does it mean? Had we not read Kugler's elaborate hypothesis, we should have corrected it without discussion as the simplest of scribal errors. Since the colophon is on the "Seitenrand," we should have first supposed that the plural sign after *šarru* (written ideographically and so by itself either singular or plural, nominative, genitive, or accusative according to the following sign or from the context) had been worn off. As for the supposed *aplani(pl)*—better read as *mare(pl)*—it would be sufficient to place these two signs by the side of the normal *maru-šu*, "his son," to discover that only the addition of two tiny wedges is needed to turn the *šu* into the plural sign. As the usual Seleucid script of the astronomical tablets is minute and cramped, we should conjecture the error was made by the modern copyist; for with full recognition of the difficulty of such copying and of the extraordinary skill of Strassmaier in deciphering these tablets, Kugler himself repeatedly corrects Strassmaier's copies. We therefore restore the colophon in accordance with the usual formulas, *šattu 104(kam) [(m)An-ti'-uk-su u (m)An-ti'-]uk-su maru-šu šarrani [(pl)]*, "Year 104, [Antiochus and Antiochus, his

<sup>45</sup> *Moses*, pp. 324 f.

son, king[s.]" Antiochus Epiphanes disappears and we have a certain date, 208/7, for the association of the younger Antiochus.

This Antiochus last appears year 119, Tabitu 21 (January 28, 192 B.C.).<sup>46</sup> From year 120, Tashritu 2 (October 2, 192 B.C.), to year 122, Tabitu 9 (January 13, 189 B.C.), Antiochus III had no associate, but by year 123, Tashritu 14 (October 11, 189 B.C.), his second son, Seleucus, was his associate.<sup>47</sup> The last date of Antiochus and Seleucus is year 124, Kislimu 4 (December 18, 188 B.C.).<sup>48</sup>

The first date of Seleucus IV is year 125, Duzu 11 (July 20, 187 B.C.).<sup>49</sup> A double date, "year 68 which is 132, Arshakan, which . . . , " obviously equates Seleucid and Parthian eras but certainly does not prove a temporary Parthian conquest of Babylonia in the year 180/179 B.C.; if not an error for 168 Parthian era, 232 Seleucid era, 80/79 B.C., it is backdating by the scribe, exactly as we date years "Before Christ."<sup>50</sup> The last certain dating of Seleucus IV is year 133, month lost, day 16, 179/78 B.C.<sup>51</sup>

It has recently been suggested that Antiochus Epiphanes reigned for a time as regent for the child Antiochus, son of Seleucus IV; whatever may be said of the dubious literary and numismatic evidence, it is incorrect to claim in further support that "cuneiform documents from the first year of Antiochus to the year 169 have in their dating 'Antiochus and Antiochus kings.'"<sup>52</sup> This is definitely disproved by an unpublished Oriental Institute tablet, A. 2518, year 137, Addaru 10 (February 27, 174 B.C.), where only Antiochus is king.<sup>53</sup> Two Antiochi do appear in year 138, Airu 22 (June 7, 174 B.C.), but give no support

<sup>46</sup> Schroeder, No. 32.

<sup>47</sup> *Ibid.*, Nos. 47, 14; Clay, *BRM*, II, 14.

<sup>48</sup> Thureau-Dangin, *op. cit.*, No. 24; citations of the whole year 124 to these and of 125 to "An and Si his son, kings" (Strassmaier, *ZA*, VIII, 109), are only from astronomical tablets.

<sup>49</sup> Clay, *BRM*, Vol. I, No. 88; curiously enough, Kugler (*Moses*, p. 322, n. 2) argues that the double occurrence of "Si king" for year 125 is wrong as compared with another date 126.

<sup>50</sup> Strassmaier, *ZA*, VIII, 110; Kugler (*SK*, II, 448, n. 1) considers it an error.

<sup>51</sup> Clay, *BRM*, Vol. II, No. 37; citations for 134-136, 178/77, 176/75 are astronomical, and the last was calculated ahead.

<sup>52</sup> E. R. Bevan, *Cambridge Ancient History*, VIII, 498.

<sup>53</sup> Speleers, Nos. 298, 300, cited by Krückmann (*loc. cit.*) as of year 136, actually are of year 96, and so of Antiochus III.



to the hypothesis,<sup>54</sup> for the very next year, 139, Abu 12 (August 14, 174 B.C.), we are specifically told it is "Antiochus and Antiochus, his son, kings."<sup>55</sup> Oriental Institute A. 3679, year 140, Tashritu 30 (broken but fairly certain) (November 6, 171 B.C.) has the two rulers; so probably from the space also A. 3688 from the next year.<sup>56</sup> By 144, Duzu 12 (July 20, 168 B.C.), however, Antiochus is alone, and thenceforth until 150 we have sufficient tablets to show that he continued alone.<sup>57</sup> That contemporary Babylonian tablets should consistently fail to mention the future Antiochus V from 169 to 162 B.C. is passing strange but cannot be explained from Babylonian texts, astronomical or otherwise. Clay assumed that this single Antiochus indicated the beginning of the reign of Antiochus V in 169 B.C., and this supposition has been followed by Krückmann; but Kugler rightly objects that during the first years of this period Antiochus Epiphanes was very much alive and on the throne,<sup>58</sup> in fact, just at this time he was persecuting the Jews. However, Kugler quotes a table of Mars from years 136 to 146; of the preserved dates only in 144 is there added to the date by the Seleucid era a royal name, "An, king," which he explains as proving that in this year 168/7 the son was not associated. In another Mars tablet, prepared in the year 194, 118/7 B.C., one line has "year 115 (197/6 B.C.) An and An, his son, king"—of course, Antiochus III and his son; in the next line "year 147," 165/4 B.C., is not followed by a royal name, and this, according to Kugler, proves that the formula in

<sup>54</sup> Clay, *BRM*, II, 14; Kugler (*SK*, II, 441), year 138, Nisannu 1 (April 17, 174 B.C.) is astronomical.

<sup>55</sup> Clay, *BRM*, Vol. II, No. 38; omission of the son for this year 139 in the list of Strassmaier (*ZA*, VII, 330) means nothing.

<sup>56</sup> Schroeder, No. 17, Year 143, Shabatu 21 (March 3, 168) probably had the royal name in the broken beginning of the second line of the formula. Kugler (*ZA*, XV [1900], 191, *Moses* (p. 328), Strassmaier (*ZA*, VIII, 110), and Clay (*BRM*, II, 14) all refer to one eclipse tablet for year 142, Tabitu 29 (January 23, 169 B.C.) with "An and An, his son, kings." The "Antiochus and Antiochus, his son, kings," of Epping-Strassmaier (*ZA*, VI, 218) is also computed.

<sup>57</sup> Contenau, No. 245. Other cases where Antiochus is alone are Schroeder (No. 30), year 144, Simanu 25 (July 3, 168 B.C.) and (No. 33) year 145, Addaru 13 (March 4, 166 B.C.) where text reads only (m)An-[ti'-uk-su], but there seems no room for more; Clay (*BRM*, II, 14), year 146, Ululu 12 (September 23, 166 B.C.); Schroeder (No. 13), year 146, Shabatu 7 (February 15, 165 B.C.); Reisner (*Hymnen*, No. 1), year 148, Addaru 22 (March 9, 163); Clay (*BRM*, Vol. II, No. 39), year 149, Shabatu 1 (February 9, 162 B.C.); year 150, Tashritu 18 (October 17, 162 B.C.).

<sup>58</sup> Clay, *BRM*, II, 14; Krückmann, *op. cit.*, p. 22; Kugler, *Moses*, pp. 328 f.



the second is the same as the first and so the future Antiochus V must have been again associated. Aside from the fact that the Babylonians often employed the ancestor of our ditto mark for this purpose, the contemporary records quoted above prohibit what in itself was a very improbable hypothesis.<sup>59</sup>

For the reign of Demetrius I we have a fairly good series, the earliest in the colophon of a contemporary astronomical tablet dated at Babylon, "year 151, Tabitu 4, Dimitri king," January 18, 160 B.C.; one of year 154, 158/57 B.C., apparently has no mention of a king. The last is year 161, 151/50 B.C.<sup>60</sup> Alexander Balas appears in year 162, Tashritu 5 (October 21, 150 B.C.); in year 164, Kislimu 16 (December 8, 148 B.C.) the unnamed ruler is called only "king of the Babylonians," hinting perhaps of revolt or doubt of the true monarch; his last tablet is year 166, Arahsamna 20 (November 19, 146 B.C.).<sup>61</sup> For Demetrius II we have little good information; only the horoscope of year 169, Addaru 6 (March 1, 142 B.C.) is contemporary.<sup>62</sup>

At no time do we regret more the distraction of Kugler from his true work than when we read the extract from the astronomical document which tells of the Parthian conquest, to have been published in the third volume of his *Sternkunde*,<sup>63</sup> for it is contemporary. We miss the market prices, so needed for economic history, but even the little that is given is of extraordinary value. Kugler gives no translation, only a transliteration; in the absence of the cuneiform text, restoration and translation are highly dangerous, and the following attempt is given only to enforce the need of an adequate publication by a competent Assyriologist:

... Men of all sorts [Demetrius collected,] to the cities of Media [he marched . . . ] that month, on the twenty second . . . the *rab uqu*, (general,) entered the land of Akkad. [Against him] Arshaka (Arsaces), the king, to the city of Seleucia [went, the city of . . . of] the land of Ashur (Assyria), which before the face of king Arshaka [had bowed down, . . . into

<sup>59</sup> *Moses*, pp. 328 f.

<sup>60</sup> Kugler, *Moses*, p. 334; Clay, *BRM*, Vol. II, No. 45; Contenau, No. 246 (name of king missing).

<sup>61</sup> Clay, *BRM*, II, 14; Strassmaier, *Actes du Huitième Congrès International des Orientalistes* (1893), II, Part IB, pp. 281 ff.; Clay, *BRM*, Vol. II, No. 50.

<sup>62</sup> Strassmaier, *ZA*, III, 137, 149 f.; Kugler, *Moses*, p. 335; Strassmaier (*ZA*, VIII, 111) gives year 168, 144/3 B.C.

<sup>63</sup> *SK*, II, 442; *Moses*, pp. 338 ff.

the city of Seleucia, the royal city, he entered, that month, on the twenty-eighth, [he sat on the throne]. Year 171 Arshaka, the king, on the 30th of Duzu (August 8, 141 B.C.) . . . .

Then follow astronomical data according to Kugler. Later he read: "That month"—Ululu, August, or later—"on the third day, Nica[tor the king was made prisoner(?) . . . ] Arshaka, the king . . . the city Seleucia."<sup>64</sup>

Mithradates I conquered Seleucia before the solar eclipse of year 171, Duzu 13 (July 22, 141 B.C.); presumably, therefore, the enthronement was Simannu 28 (July 28, 141 B.C.). If the following reference to Nicator is to the captivity of Demetrius, this took place Ululu 3 (September 21, 141 B.C.) or an equivalent month later. Another astronomical document with data for Kislimu and Tabitu of this year (from December 5, 141 B.C., to February 1, 140 B.C.) mentions the king's departure to Arqania (Hyrcania), a battle of the Elamite before Apam'a (Apameia) on the river Silhu (Sellas of Messene).<sup>65</sup>

Our first Parthian business document was written little more than two months after the enthronement, Arisakka' king, years 107-171 (October 13, 141 B.C.).<sup>66</sup> That we have at once double dating, by the Arsacid era beginning Nisannu 1 (April 14, 247 B.C.)<sup>67</sup> proves that the former era was already in use before the conquest of Babylonia. Next year, 108 Arsacid, 140/39 B.C., begins the titular "king of kings."<sup>68</sup> In the following, year 109-173, 139/38 B.C., the formula is "Arisak', the king"; the same scribe writes a tablet whose formula is "Arshak' and Ri-[in(?)]-nu, his mother, kings," date lost, but the reference to the queen mother shows the king a youth, Phraates II, who became king about 138 B.C.<sup>69</sup> A curious "record of debt" dated "year six Ar'siuqqa, king," would be placed in 133 B.C.

Parthian conquest brought a revival of copies of ancient literature, especially astronomical, which now have contemporary date formulas.

<sup>64</sup> The historical conclusions will be presented in the forthcoming *Political History of Parthia* by Dr. N. C. Debevoise.

<sup>65</sup> Pinches, *Old Testament*, pp. 484, 553; *Moses*, pp. 338 ff.

<sup>66</sup> Schroeder, No. 37.

<sup>67</sup> Kugler, *SK*, II, 443.

<sup>68</sup> Strassmaier, *ZA*, III, 130, 143.

<sup>69</sup> Clay, *BRM*, Vol. II, Nos. 52 f.; cf. pp. 13, 33 f.

They come to a temporary close in year 116-180, 132/1 B.C.<sup>70</sup> The sole record of the temporary reconquest of Babylonia by Antiochus VII is a copy of an ancient hymn, year 182, Airu 22 (June 1, 130 B.C.).<sup>71</sup> It is the last witness to Western rule; it is also strong evidence for the survival of the oriental spirit. Next, Babylonia fell into the hands of Aspasine of Charax, as a letter dated year 185, Airu 24 (June 1, 127 B.C.) proves,<sup>72</sup> but in year 122-186, 126/5 B.C., astronomical texts were once more dated by Arsaces the king.<sup>73</sup>

In their formulas the Babylonians living under Parthian rule used only Arshaka, Arshakan, or Arshakamma (Arsaces in Greek), like the Roman Caesar, a mere official title, unless there was a rival to the throne, when the personal name might be employed. As a result, the Babylonian tablets give little aid in determining the Parthian succession, and henceforth our reliance is on the coins. The few cases where the tablets present additional information can be understood only where their data are brought into connection with the general history, and these cases may be left for discussion by Dr. N. C. Debevoise in his forthcoming *Political History of Parthia*.

It is no reproach to students of Hellenistic history that they have been misled by those who should have known better. The purpose of the present study is not so much to bring the evidence up to date as to test what has been already utilized and to clear the ground for the new tablets of the Seleucid period which some day will be published.

Details of chronology tend to be dull, but the conclusions are fundamental. There are more interesting, though not more important, additions to our knowledge of the Hellenistic civilizations still hidden in cuneiform tablets.

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<sup>70</sup> Kugler, *SK*, II, 446.

<sup>71</sup> Reisner, *op. cit.*, No. 25; this follows the cuneiform text. In the introduction the date is given year 183, which would make it May 19, 129 B.C.

<sup>72</sup> Pinches, *Babylonian and Oriental Record*, IV (1890), 131 ff.

<sup>73</sup> Kugler, *SK*, II, 446.

## THE PEACE OF PHOENICE AND THE OUTBREAK OF THE SECOND MACEDONIAN WAR

JAKOB A. O. LARSEN

IN HIS study, "Les préliminaires de la seconde guerre de Macédoine,"<sup>1</sup> E. Bickermann has made some interesting and valuable contributions to the interpretation of the old problem of the outbreak of the Second Macedonian War. Unfortunately only the first instalment was available at the time of the publication of my article, "Was Greece Free between 196 and 146 B.C.?"<sup>2</sup> and even it came too late to be given the consideration it deserved. Consequently, it may not be out of place to consider some of the points raised by his article.

Bickermann's study centers round the Peace of Phoenice of 205. The *foederi adscripti* of the treaty are explained in the light of Greek institutions, and the peace is brought into connection with the *κοινὴ εἰρήνη* of fourth- and third-century Greek history. There is, however, an important departure in procedure. Earlier treaties of the kind were collective treaties. The Peace of Phoenice was a dual treaty between Rome and Philip with the other parties interested merely included among the *adscripti*. In a peace of this nature it would be possible for any interested state to be included. Thus it is no longer possible to object to the authenticity of the list of states which, according to Livy xxix. 12. 14, was appended to the treaty by the Romans on the ground that Rome would not have included neutrals. In the opinion of Bickermann, in fact, all states listed, except Pergamum, were neutral.<sup>3</sup> Except for a slight doubt concerning Ilium he has no hesitation in accepting the entire list.<sup>4</sup> With the question of the nature of the treaty

<sup>1</sup> *Revue de philologie*, LXI (1935), 59-81 and 161-76. In the preparation of the present paper a reprint kindly sent by the author has been used. References are given by the page numbers of the periodical with the pages of the reprint given in parentheses.

Bickermann has made one very unfortunate error. On pp. 63 (5) and 167 (29) he refers to the negotiator of the Peace of Phoenice as P. Sulpicius instead of P. Sempronius Tuditanus (Livy xxix. 11. 10; 12). Though this results in the identification of the negotiator with the consul of 200 B.C., the mistake does not affect seriously the chief points at issue.

<sup>2</sup> *Class. Phil.*, XXX (1935), 193-214.

<sup>3</sup> P. 67 (9).

<sup>4</sup> P. 68 (10).

disposed of, the author then proceeds to demonstrate that the Roman intervention in Greece in 200 B.C. was based on the treaty. Rome did not desire war but at the outset merely made the minimum representations required by the treaty. No more did Philip desire war. Thus the conflict was not due to an act of will on the part of either one of the major contestants but was the result of a chain of circumstances which in the end made hostilities unavoidable.

In many respects Bickermann's interpretation is attractive. It makes it easy to explain why the Roman diplomatic and military intervention got under way so slowly; and the fact that Philip understood the attitude of the Romans in turn explains his failure to make earlier preparations to meet the Roman offensive. Nevertheless, on examination it has seemed to me that Bickermann is mistaken on two major points: (1) The *adscripti* included in the treaty by the Romans were not neutrals but allies possessing formal treaties of alliance with Rome. (2) The first Roman ultimatum presented to the Macedonians was not based exclusively on the Treaty of Phoenice.

To begin with the problem of the *adscripti*, it is natural for one who, like Bickermann, has built upon the foundations laid by Holleaux to classify the states in question as neutral, and, furthermore, it is natural for anyone who approaches a problem connected with the Peace of Phoenice to build upon the brilliant work of Holleaux. Nevertheless, in my opinion, an examination of the evidence leads to the conclusion that the list of the Roman *adscripti*, in as far as it is authentic, contains states allied with Rome. Sparta, Elis, and Messene have already been discussed.<sup>5</sup> This leaves Ilium, Pergamum (Attalus), the Illyrian kingdom (Pleuratus), and Athens to be considered. Of these it will be seen that Attalus and Pleuratus almost certainly were allies while Ilium and Athens do not belong in the list. Their insertion is a part of that corruption or falsification of the sources which has affected our accounts of the period also at other points.

The case of Pergamum is not quite so clear as that of Sparta, Elis, and Messene. It was listed among potential allies in the Romano-Aetolian treaty of 212.<sup>6</sup> There is no direct evidence for the actual

<sup>5</sup> *Class. Phil.*, XXX, 210-12.

<sup>6</sup> Livy xxvi. 24. 9.

making of the treaty, but the existence of a treaty between Rome and Pergamum has been deduced from the fact that, when Roman and Pergamene forces co-operated during the Second Macedonian War, the spoils were divided in a manner much like the arrangements made between the Romans and Aetolians in 212.<sup>7</sup> Then, too, it would have been surprising if treaties had been made with Messene and Elis but not with Pergamum. It is likely that the Illyrian kingdom also possessed a formal treaty of alliance. Scerdilaidas and Pleuratus had been included in 212 among the potential allies, that is, they had been offered the opportunity of transforming the *amicitia* resulting from the treaty of 228 and, no doubt, renewed after the Second Illyrian War into a formal alliance. This is not surprising. Though the Illyrian princes did not always live up to the standard of *amici* of Rome, nevertheless, at the time of the outbreak of the Second Macedonian War, Scerdilaidas was active against Philip.<sup>8</sup> The mere fact that the Romano-Aetolian treaty of 212 was followed by something like wholesale treaty-making suggests that also the projected treaty with the Illyrian kingdom actually was concluded. A further argument can be found in the difference between the treatment accorded Pleuratus and the treatment accorded other Illyrian states in the Peace of Phoenice itself. While the clause that applied to other states merely provided *ut . . . Romanorum essent*, Pleuratus was listed among the *adscripti* alongside of the allies of Rome.

The evidence for Attalus and Pleuratus may not be entirely convincing when taken by itself. Yet, when it is remembered that three of the states included in the list already have been shown to have been

<sup>7</sup> Holleaux, *Rome, la Grèce et les monarchies hellénistiques*, p. 217, n. 2; cf. *CAH*, VIII, 130. Note particularly in connection with the capture of Oreus in 199: "Urbs regi, captiva corpora Romanis cessere" (Livy xxxi. 46. 16). The conclusion of Holleaux seems justified even though, in the settlement of 196, Oreus and other cities of Euboea were not left in the hands of Eumenes but were set free (Polyb. xviii. 47. 10-11; Livy xxxiii. 34. 10). In opposition to the commissioners, who favored giving Oreus and Eretria to Eumenes, Flamininus probably argued that the old order of things represented by the treaties had been superseded by the new plan of the freedom of the Greeks. It might even have been argued that Attalus and Eumenes had declared their adhesion to this principle by their co-operation with the Romans. Nevertheless, it seems that Eumenes was permitted to retain one of the conquests made during the war, namely, Andros. The island is not mentioned among the states liberated, and there is extant a fragmentary inscription (Th. Sauciuc, *Andros*, No. 3) which seems to imply that Andros was subject to Pergamum.

<sup>8</sup> Holleaux, *op. cit.*, pp. 165, n. 4, 166 ff., and *passim*.

allies of Rome, it is sufficient to make it reasonably certain that Pergamum and the Illyrian kingdom also were allies. Even if one chooses to class them as *amici* without treaties of alliance, it must be admitted that the two kings had been so closely associated with the Romans that their inclusion among the *adscripti* is perfectly natural. The admission that they may have been merely *amici*—though, in my opinion, such an admission is unnecessary—would not prove that a place among the *adscripti* was open, as it were, to all comers. It would rather prove that the treaty did not differentiate between actual allies and *amici* that had taken the side of Rome in the war. Thus, as far as the five states hitherto considered are concerned, the *adscripti* remain essentially allies of Rome, and, with this reservation, they will hereafter be treated as such.

The case of Athens and Ilium is entirely different. Athens certainly did not possess a treaty of alliance but, at best, *amicitia* with Rome. Aside from the question of the reliability of the annalistic sources used by Livy in the first part of Book xxxi, it is necessary to remember that the words *socii* and *societas* are used so loosely that the occurrence of these words is no proof of the existence of a treaty.<sup>9</sup> The evidence for the early relations between Athens and Rome is given by Holleaux in his article, "Le prétendu recours des Athéniens aux Romains en 201/200."<sup>10</sup> Holleaux himself considers *amicitia* a product of definite treaties and so denies even its existence, but if "*amicitia* is the product of any non-hostile intercourse between Rome and a foreign state,"<sup>11</sup> then the embassy of 228<sup>12</sup> was enough to produce this relation between the two states. Yet such an isolated act need not lead to any close association, and we may well believe with Holleaux that Rome did not consider the participation of Athens in the efforts to mediate between Philip and the Aetolians during the First Macedonian War an act of friendship. Thus it is very unlikely that Rome in 205 should wish to list Athens alongside of her own allies.

Even so, according to Bickermann's interpretation of the treaty, the inclusion of Athens was not impossible. Since the earlier relations

<sup>9</sup> Matthaei, *Classical Quarterly*, I (1907), 186.

<sup>10</sup> *Revue des études anciennes*, XXII (1920), 77-96 at pp. 93 f.

<sup>11</sup> *Class. Phil.*, XXX, 195.

<sup>12</sup> Polyb. ii. 12. 8.



of Rome and Athens were not close, however, such an inclusion would have meant a startling innovation in the foreign policies of both states. This innovation, in turn, would have left its mark on their future relations. In other words, if it is true that Athens sent repeated embassies to Rome before the outbreak of the Second Macedonian War, it is natural to believe that she actually was included in the Treaty of Phoenice. Thus the solution of the problem depends on the historicity of the Athenian embassies to Rome reported by Livy.

The most important recent discussions of this question are by Holleaux, De Sanctis, and Passerini.<sup>13</sup> The sources are given by Holleaux (pp. 82-83). To these should be added Livy xxxi. 14. 3, which relates that an Athenian embassy met Sulpicius after he had crossed from Brundisium to "Macedonia." The account of Appian<sup>14</sup> is so brief and confused that it is practically without value in spite of the fact that it contains material ultimately derived from Polybius. This leaves Livy and Pausanias to be considered.

To my knowledge no one has challenged the general interpretation of Nissen,<sup>15</sup> who has shown that the first part of Livy xxxi is derived from annalistic sources but that in chapter 14 the author begins to follow Polybius. According to the annalistic sources, two Athenian embassies reached Rome before the proposal to make war was submitted for the first time to the *comitia* only to be rejected by it. On the other hand, in Livy xxxi. 14 it is reported that when Sulpicius had crossed to "Macedonia" he was met by Athenian ambassadors who requested that he relieve their city, which was being besieged. Thereupon the author proceeds to tell how Athens came to be at war with Philip. In this account he draws upon Polybius, and it looks very much as if the latter had related the steps that led to the Roman declaration of war without mentioning any embassies from Athens. It is true that Roman ambassadors were present at Athens when the Athenians, urged on by Attalus and the Rhodians, declared war

<sup>13</sup> Holleaux, article cited p. 18 and n. 10; De Sanctis, *Storia dei Romani*, Vol. IV, Part I, chap. i; Alfredo Passerini, "Studi di storia ellenistico-romana," *Athenaeum*, IX (1931), 260-90 and 542-62. These three accounts will be cited hereafter merely by the names of the authors.

<sup>14</sup> *Mac.* 4. 2; cf. Holleaux, pp. 84-87.

<sup>15</sup> *Kritische Untersuchungen über die Quellen der vierten und fünften Dekade des Livius*, pp. 119-25.

against Philip. Attalus even implied that the Romans would take part in the war,<sup>16</sup> but the Romans decidedly kept in the background<sup>17</sup>—conduct that certainly would be most surprising if Athens already had twice appealed to Rome. Furthermore, the fact that Athens is not mentioned in the Roman ultimatum first presented to Nicanor and later to Philip in person<sup>18</sup> makes it seem very unlikely that the quarrel between Athens and Philip was an important consideration to the Romans and consequently also tends to show that the Athenian embassies that are supposed to have influenced the Romans are not historical. It is true that, if adequate proof of the historicity of the embassies can be found, the action of the Romans in both cases can be explained plausibly.<sup>19</sup> Nevertheless, the incidents, taken by themselves, tend to discredit the historicity of the Athenian embassies reported by the annalistic sources of Livy.

Pausanias i. 36. 5-6 remains to be considered. In connection with a reference to the monument of Cephisodorus the author speaks of the opposition of this statesman to Philip and tells that he secured or attempted to secure aid.<sup>20</sup> When the aid from Egypt, Mysia (Attalus), and the Cretans came too late and the Rhodians with their ships were of little help against the Macedonian hoplites, then Cephisodorus with other Athenians sailed for Italy and asked the Romans to help. This passage has been used to prove the historicity of the appeal of Athens to Rome, but it shows clearly that this appeal was made at a relatively

<sup>16</sup> Polyb. xvi. 26. 6; Livy xxxi. 15. 4.

<sup>17</sup> Polyb. xvi. 25-26, reproduced with omission of all references to Roman envoys by Livy xxxi. 14-15.

<sup>18</sup> Polyb. xvi. 27. 2; 34. 3-4.

<sup>19</sup> De Sanctis, p. 32, n. 65.

<sup>20</sup> *συμμάχους δὲ ἐπήγετο Κηφισόδωρος Ἀθηναίους γενέσθαι βασιλεῖς μὲν Ἀτταλον, κ.τ.λ.* Jones (Loeb ed.) translates: "Cephisodorus induced to become allies of Athens, etc." I prefer a looser rendering for two reasons:

1. The imperfect in the case of *ἐπήγετο* may be conative. Thus the author does not state clearly that all appeals were successful.

2. The words "to become allies" translate perfectly *συμμάχους . . . γενέσθαι*. If these words are construed strictly, they can be taken to refer to the making of treaties of alliance. Such a literal interpretation, however, is scarcely warranted. What Cephisodorus asked for was help and not treaties. Holleaux (p. 84, n. 2) construes too closely when he objects to the passage on the following ground: "Il est trop évident . . . que les Athéniens n'ont, dans les circonstances indiquées par Pausanias, conclu d'alliance' ni avec Ptolémée, ni avec les Aitoliens, ni avec les Crétois, ni même avec Attale et les Rhodiens."

late date. The account of Pausanias cannot well be reconciled with the annalistic account in Livy which reports the arrival at Rome of two Athenian embassies before the question of war was submitted to the *comitia* for the first time. Thus, in my opinion, Holleaux,<sup>21</sup> when he rejected the evidence of Pausanias, rejected the particular evidence which supplies the strongest argument in favor of his own general point of view.

On the other hand, Niese,<sup>22</sup> who has been followed by others, recognized that Pausanias derived his material from the inscription on the monument of Cephisodorus. Thus the document quoted by Pausanias is the one primary source that we have for the relations of Athens and Rome at the time. Unfortunately, he does not himself state that he is citing the inscription. Though it is difficult to see from what other source he can have derived his detailed account of Cephisodorus, nevertheless it is desirable to have some further demonstration of the value of his evidence. This has been supplied by Passerini,<sup>23</sup> who has found support for the account of Pausanias in Livy xxxi. 9. In this chapter it is reported that, when Rome was in the midst of preparations for war after the favorable vote in the *comitia*, an embassy from Ptolemy arrived. This embassy reported that Athens had appealed

<sup>21</sup> P. 84 and nn. 2 and 3.

<sup>22</sup> *Geschichte der griechischen und makedonischen Staaten*, II, 590, n. 1. Cf. Ferguson, *Hellenistic Athens*, p. 269, n. 1; De Sanctis, p. 21, n. 53; *Class. Phil.*, XXX, 212, n. 82.

An Athenian decree of 196/5 B.C. honoring Cephisodorus was discovered during the excavations in the Agora in 1933 and has recently been published by B. D. Meritt (*Hesperia*, V [1936], 419-28). The honors voted include two bronze statues to be erected in the Agora and the Emporium of the Peiraeus. The inscription cited by Pausanias is neither the present decree nor the inscription on the base of either one of the statues to be erected but the inscription on the tomb (*μνημα*) of Cephisodorus on the Sacred Way. The decree refers to his services as ambassador only in general terms and so neither contradicts nor substantiates directly the theory that Cephisodorus was sent to Rome at least twice. Yet some support can be derived from the statement that he "has explained how the Demos might keep firm in their faith existing friends and gain also others in addition; has foreseen the plots being prepared by outsiders and has set himself to oppose them; and has recommended good alliances advantageous to the Demos; and has gone on embassies of the greatest importance for the safety of the cities and the countryside" (Meritt's translation). The new friends that he secured must be the Romans, and the statement concerning them makes best sense if Cephisodorus not only directed the policy of Athens at the time but also served on the embassy that first secured Roman aid for Athens. Nor does the reference to his embassies contradict the theory of the two embassies. The plots of outsiders that he frustrated must be the plots of Philip. Cf. the commentary of Meritt, who remarks: "The whole passage must be read in the light of Pausanias i. 36. 5."

<sup>23</sup> Pp. 283-85.

for help against Philip and asked for instructions concerning the course of action to be pursued. Would the Romans defend their *socii*<sup>24</sup> or was Ptolemy to send help? The report of this embassy obviously contradicts the general tenor of this section of Livy in which it has been reported that two Athenian embassies came to Rome before the war motion was submitted to the *comitia* for the first time, and further that the appeal of the Athenians was influential in inducing the Romans to go to war.<sup>25</sup> For this very reason it deserves credence. Furthermore, it fits admirably with the account of Pausanias, and thus these two bits of evidence, preserved in such totally different ways, support each other and make it possible to reconstruct the true course of events. When Athens became involved with Macedonia, she appealed at first only to Greek and Hellenistic states and sent no embassies to Rome before the Romans themselves had decided to declare war on Philip. Instead, the first appeal on behalf of Athens was made by Ptolemy after the *comitia* had already voted for war. It is true that this interpretation deals rather boldly with the account of Livy, but it must be remembered that all investigators, no matter what interpretation they adopt, agree that this section of his work is full of mistakes. The only procedure that gives much hope of sifting the wheat from the chaff is a comparison with other sources, and the one source that offers the best help is Pausanias. The authenticity of the latter source, in turn, is supported by the fact that it agrees with an incident reported by Livy which contradicts the rest of his narrative and so cannot have been included for any other reason than that it happens to be true. Finally, it should be noted that the interpretation adopted agrees with the conclusions already reached by some scholars on the basis of the account of later events given by Polybius. De Sanctis, to be sure, has given a different interpretation of the evidence in Polybius, but his interpretation, it already has been implied, can be defended only if there is authentic proof in other sources for the early embassies of Athens to Rome. It has now been shown that such evidence does not exist.

The embassy of Cephisodorus reported by Pausanias still remains to be dealt with. It has been suggested that the statement concerning

<sup>24</sup> Need it be repeated that the use of this word does not prove the existence of a treaty of alliance?

<sup>25</sup> Livy xxxi. 1. 10; cf. *ibid.* 7. 6.

this embassy is due to a confusion with the later embassy of Cephisodorus reported by Polybius.<sup>26</sup> Even such a mistake as this on the part of Pausanias would not prove that his account is not based on an inscription. Yet the fact that Cephisodorus went to Rome a couple of years later does not prove that he did not serve as an ambassador also in 200. In my opinion Passerini has found the correct solution when he connects the embassy of Cephisodorus with the one reported in Livy *xxi. 14. 3*. When Sulpicius reached "Macedonia" on the second day after he sailed from Brundisium, he found there an Athenian embassy which requested relief from siege. It is true that Pausanias reported that Cephisodorus sailed for Italy. This does not involve any contradiction. It might even be argued that it is not stated that he reached Italy. Obviously he left Athens some time before Sulpicius started from Brundisium, nor can the Athenians have known what the plans of Sulpicius were. Under the circumstances his objective when he left Athens must have been Italy or Rome itself. Naturally, when he found Sulpicius crossing the Adriatic, he approached him. The account of Livy does not state whether he continued on his way to Italy. Finally, it must be noticed how closely the two accounts agree on two essential points. Pausanias tells that Cephisodorus went in search of aid, and he obviously was praised on the monument for having secured this.<sup>27</sup> The embassy reported by Livy requested aid and secured it. In the second place, Pausanias reports that Cephisodorus appealed to the Romans for the reason that the aid rendered by the Rhodians with their ships was of no avail against the Macedonian hoplites. This implies that the embassy set out at the time of a large-scale Macedonian offensive against Athens. The embassy mentioned by Livy reported that Athens was being besieged. It is difficult to avoid the conclusion that the two accounts are dealing with the same embassy. Apparently this was the first embassy sent by Athens to Rome or a Roman general, and it was sent at a time when the Athenians already must have known that Rome had declared war on Philip.

The fact that an Athenian embassy actually was dispatched, even though this took place at a late date, makes it clear how easily tradition could be distorted. All that was necessary was to antedate and

<sup>26</sup> xviii. 10. 11; cf. Holleaux, p. 84, n. 3, and the Hitzig-Blümner, Frazer, and Jones editions of Polybius.

<sup>27</sup> The remarks of Pausanias concerning the ultimate results of the Roman intervention cannot have been taken from the inscription.

give a false importance to a genuine historical event. Furthermore, even though no Athenian embassy had arrived before the *comitia* voted in favor of war, and even though Athens was not mentioned in the Roman ultimatum, it is still possible that the Roman voters really were warned that, if they failed to protect Athens as they had failed to relieve Saguntum, there was danger that Philip might invade Italy like a new Hannibal or, at least, a new Pyrrhus.<sup>28</sup> If this were the case, it would be very easy to believe that the Athenian embassy must have arrived before the argument was used. But though the embassy as a result was transferred to an earlier date, a record of it at the proper time also remained and the same embassy was thus reported twice. When this step had been taken, it was easy to take one more step and to represent the appeal from Athens as one of the causes of the Roman intervention.

A further glance at the evidence will, in my opinion, show that all reports of the Athenian embassies are ultimately derived from the embassy that met Sulpicius after he had crossed the Adriatic. The reference to the *preces Atheniensium* in Livy xxxi. 1. 10 is very general and might well apply to the embassy reported in 5. 5-6, which, it must be remembered, arrived before the first vote in the *comitia*. The only justification for maintaining that a new embassy is involved is the description of the embassy in the second passage as a *nova legatio*. Here the *nova* may well have been inserted by Livy or one of his sources for the simple reason that an appeal from Athens already had been mentioned.<sup>29</sup> For the next step it is necessary to note that neither in Livy xxxi. 1. 10 nor in 5. 6 does the description of conditions at Athens fit the time to which they are applied. In the first passage we read: "Vacuos deinde pace Punica iam Romanos et infensos Philippo . . . preces Atheniensium, quos agro pervastato in urbem compulerat, excitaverunt ad renovandum bellum" (1. 9-10); in the second: "Atheniensium nova legatio venit, quae regem appropinquare finibus suis nuntiaret, brevique non agros modo, sed urbem etiam in dicione

<sup>28</sup> Passerini (pp. 555 f.) argues that the speech of Sulpicius in Livy xxxi. 7 deserves serious consideration for the reason that it contradicts the rest of the annalistic tradition on several points.

<sup>29</sup> Cf. De Sanctis (p. 32, n. 65): "Le ambascerie atenesi sarebbero state due prima dell'inizio della spedizione di Galba, secondo Livio, ma vi è probabilmente reduplicazione."



eius futuram, nisi quid in Romanis auxilii foret" (5. 5-6). Whatever system of chronology is adopted for this difficult period, it is scarcely possible to maintain that Athens, before the first vote of the *comitia*, could have been reported as besieged and in danger of capture at the hands of Philip. On the other hand, the conditions described correspond closely to the situation reported by the ambassadors who met Sulpicius "orantes, ut se obsidione eximeret" (14. 3). When the reports are compared, it is difficult to escape the conclusion that all three are concerned with the same embassy, though the accounts in 1. 10 and 5. 6, with their references to the personal presence of Philip, have been colored by the later attack of the king on Athens.<sup>30</sup>

Since Athens made no appeal to Rome when her trouble with Macedonia began, it is clear that she was not included by the Romans in the Peace of Phoenice. It was stated above that the listing of Athens in the treaty alongside of the allies of Rome would have constituted a startling departure in the foreign policy of the two states and would have left its marks on their future relations. No such marks are found. There is nothing in the events of the years 205-200 to indicate that there was a new intimacy between Athens and Rome, or that Athens felt that she had any special claim to consideration or protection. Furthermore, if Athens was not included, it is scarcely necessary to discuss Ilium. There is even less reason to believe that the latter city had any connection with the treaty and even easier to understand how its name later came to be inserted. The conclusion must be that the Roman list of *foederi adscripti* included exclusively states with formal treaties of alliance. These were Sparta, Elis, Messene, Pergamum, and the Illyrian kingdom. The insertion of Athens and Ilium in the list given by Livy must be due to distortion and invention. The distortion involved seems less surprising when it is noticed that even the

<sup>30</sup> The account of Livy xxxi. 22. 4-25. 1 shows that the attack of Philip on Athens came after the embassy reported in 14. 3 and the resulting dispatch of C. Claudius Cento to Athens. Thus, if 1. 10 and 5. 6 describe an actual embassy, it must be placed still later than the one that met Sulpicius after he had crossed the Adriatic. This is impossible, for the embassy or embassies are reported as bringing an appeal before Rome intervened. It is more likely that the passages in question merely contain a somewhat distorted account of the embassy that is correctly placed in 14. 3. The report in the latter passage, which is followed in the next sentence by the statement that Philip was not conducting the siege in person, is more accurate. Yet even here the reference to a siege is an exaggeration, but then the ambassadors themselves may well be responsible for it.



authentic part of the list is not reproduced in the form in which it was given in the original document.<sup>31</sup>

The conclusion that the states actually included by Rome under the *foederi adscripti* had formal treaties of alliance makes it possible to deduce some further information concerning the nature of the Roman protectorate. In addition to the reference to Pleuratus the document contained a special clause providing for other Illyrian groups.<sup>32</sup> In this clause the Parthinians and several communities are spoken of as though they were subject to Rome. This, as it were, defines their relation to Philip but does not mean that they were under the direct control of Rome. The history of Illyria shows that they neither had been directly annexed at an earlier date nor were annexed at this time.<sup>33</sup> Yet the manner in which these free friends of Rome are referred to in the treaty gives some idea of the way in which Rome interpreted *amicitia*. She claimed to have a right to defend her friends in the same manner as subject territory. Conversely, she must have expected subservience from these friends. Her attitude toward such states as Corcyra, Epidamnus, and Apollonia, which are not mentioned in the treaty, would no doubt be the same. Thus, if the Greeks had studied the document carefully and read between the lines—which they, of course, did not do—they might have been able to foresee how Rome later would interpret the “freedom” of the Greeks.

The conclusion that the *foederi adscripti* listed by the Romans consisted of allies and not of neutral states also affects the entire interpretation of the document and makes it less necessary to explain it exclusively in the light of Greek usage. Probably both the Epirotes,

<sup>31</sup> The words *Nabis Lacedaemoniorum tyrannus* certainly were not used in the document.

It may be noted that the foregoing conclusion concerning the composition of the list of *adscripti* appended by the Romans corresponds closely to that of Niese, *Geschichte der griechischen und makedonischen Staaten*, II, 502.

<sup>32</sup> Livy xxix. 12. 13.

<sup>33</sup> Cf. *Class. Phil.*, XXX, 198 and n. 28. The clause under consideration will seem less surprising when it is noticed that the Treaty of Apamea also spoke of the land given up by Antiochus as subject to Rome (Polyb. xxi. 43. 15; Livy xxxviii. 38. 10). Similarly both the Treaty of Apamea and the treaty at the end of the Second Macedonian War specified what territory Rome's opponent was to give up but did not state what was later to be done with this territory. As far as the treaties are concerned, the land is ceded to Rome and she is free to dispose of it as she wishes (cf. *Class. Phil.*, XXXI [1936], 342–48). Thus, the words *Romanorum essent* do not prove that Rome had annexed or intended to annex the communities to which the words are applied.

who took the initiative, and the other Greeks involved viewed it from a Greek background and were influenced by the tradition of the *κοινὴ εἰρήνη*. But the fact, noted by Bickermann, that the treaty took the form of a dual treaty between Greece and Rome introduced a new element.<sup>34</sup> After all, the treaty seemed, except for Aetolia, to include all Greek states that might be the cause of the outbreak of a new war. With the same exception all states that had opposed Macedonia were included by the Romans, who thus had the right to intervene in their interest if the treaty were violated.<sup>35</sup> On the other hand, the Romans need not have been affected at all by the Greek idea of *κοινὴ εἰρήνη*. There certainly is no reason to believe that they allowed themselves to be jockeyed into a position in which they might be compelled to interfere in order to protect the peace of Greece whether they took any interest in it or not. Their action in accepting the treaty in the form in which it was drawn up can be explained adequately by the usage, followed in the treaties with Carthage, of including allies in their

<sup>34</sup> The earlier history of the *κοινὴ εἰρήνη* is treated very briefly by Bickermann (pp. 69-71 [11-13]). The one new contribution is the theory that Antigonus Doson, in addition to his symmarchy, arranged a *κοινὴ εἰρήνη*. In my opinion this is a mistake. In support of this theory Bickermann cites Polyb. iv. 15. 10, where the Aetolians are spoken of as *σύνμαχοι* of the Achaeans and the Messenians. He does not seem to have noticed that, if there existed a symmarchy of which the Achaeans but not the Aetolians were members, and if in addition there had been arranged a *κοινὴ εἰρήνη* in which also the Aetolians had been included, then it is absurd to suppose that the existence of the *κοινὴ εἰρήνη* resulted in making the Aetolians the *σύνμαχοι* of the Achaeans. The only purpose of the arrangement presupposed by Bickermann would have to be to include in the *κοινὴ εἰρήνη* states that were not *σύνμαχοι* of the members of the symmarchy. The reference of Polybius to the Achaeans and Aetolians as allies must mean rather that there had been no formal break between the two states since their co-operation against Demetrius II and the Illyrian pirates. A second passage cited by Bickermann (Polyb. iv. 3. 8) at first glance seems more favorable to his theory. It implies that the Aetolian commander at Phigaleia could find no legitimate occupation for brigands for the reason that the *κοινὴ εἰρήνη* arranged for the Greeks by Antigonus still continued. The use of the various forms of *κοινός*, however, is so varied that it may not be necessary always to take even *κοινὴ εἰρήνη* as a technical term. Thus the reference may be merely to the general peace that resulted from the activities of Antigonus Doson. This is probably all that Polybius means. The point of this statement is merely that this peace gave no opportunity for privateering—if it is permissible to use this term concerning activities on land as well as on sea. If this state of peace must be connected with an institution, it is not necessary to look beyond the Hellenic League, which did produce a state of general peace in Greece that lasted until it was disturbed by that activity of the Aetolians which, in turn, led to the Social War. This state of peace certainly affected the Aetolians even though they themselves were not members of the organization that brought it about.

<sup>35</sup> Cf. De Sanctis, III, Part II, 436.

treaties.<sup>36</sup> In this case since the treaty obviously regulated only the affairs of Greece and adjoining countries, only Greek and Illyrian allies were included; at least, only such allies are given in the brief summary of the treaty that has been preserved.<sup>37</sup> The adoption of such a treaty must mean that Sempronius, and probably other Roman statesmen involved in the negotiations, desired to retain some hold on or connection with the Greek world.<sup>38</sup> Though Rome seems to have pursued no interest in the First Macedonian War except to keep Philip harmless, this action is not surprising. It is hardly necessary to look for any more subtle motive than the feeling that it was well to remain in touch with Philip's Greek enemies as long as he had not been decisively defeated.

Bickermann's contention that the Roman intervention in 200 was based on the Peace of Phoenice remains to be discussed. It is not necessarily invalidated by the interpretation of the treaty offered above. The Romans might well have intervened without themselves desiring war, merely because they thought it unwise not to support Attalus.<sup>39</sup> In that case it would be natural to base their intervention completely on the Peace of Phoenice and to present their demands in the most conciliatory way possible.

The question of the validity of the theory of Bickermann depends primarily on an analysis of the Roman ultimatum in the form in which it was presented to Nicanor and published throughout Greece.<sup>40</sup> The additional demands in the interest of Ptolemy and the Rhodians, included in the ultimatum later presented to Philip,<sup>41</sup> represent a gratuitous intervention on the part of the Romans and have no connection with the Peace of Phoenice.<sup>42</sup> Thus it will be necessary to take

<sup>36</sup> Cf., on the treaties with Carthage, Kolbe, "Die Kriegsschuldfrage von 218 v. Chr. Geb.," *Sitzungsberichte der Heidelberger Akademie, phil.-hist. Klasse*, 1933-34, Abh. 4, pp. 13 f.

<sup>37</sup> Bickermann remarks: "On cherche vainement sur cette liste des clients de la République, soit Naples, Tarente, etc." (p. 64 [6]). I find nothing surprising in this.

<sup>38</sup> Cf. *Class. Phil.*, XXX, 200.

<sup>39</sup> One might well argue that refusal of the Romans to give favorable hearing to an earlier appeal from Aetolia shows that they did not desire war; on this incident see Holleaux, *Rome, la Grèce et les monarchies hellénistiques*, p. 293 and n. 1.

<sup>40</sup> Polyb. xvi. 27. 2-3.

<sup>41</sup> *Ibid.* 34. 3-4.

<sup>42</sup> "C'est une intervention de Rome pure et simple, sans que rien l'y oblige, dictée donc par les raisons d'ordre politique" (Bickermann, p. 166 [28]).

for granted that we have a reasonably complete report of the original ultimatum and that this contained only two points—the demand for a settlement with Attalus and the demand that Philip abstain from war against the Greeks. The question thus becomes one of the relation of these two demands to the Peace of Phoenice. In discussing this problem it probably is best to leave aside a number of related subjects, such as the chronological problems involved and the relation of the Roman diplomacy to the fetial rites. In the present state of our knowledge a discussion of such questions is more likely to confuse than to clarify.<sup>43</sup>

The fact that Athens was not included in the Peace of Phoenice in turn proves that the ultimatum presented to Nicanor was not based exclusively on this treaty. This is important, for Bickermann<sup>44</sup> has demonstrated convincingly that the other clause of the ultimatum, the demand that Philip submit his differences with Attalus to arbitration, was based on the treaty. The Romans, however, allowed Nicanor to understand that his operations in Attica constituted a contravention of their demand that Philip abstain from war against the Greeks. By so doing they indicated clearly that the demand was meant to cover not only states included in the Peace of Phoenice but also other Greek states.

The meaning of the term "Hellenes" in the Roman ultimatum is, thus, in part indicated by the example of Athens, which shows that it included also states not listed among the *adscripti*. An effort to determine its meaning more precisely leads to the conclusion that two different interpretations appear in the diplomacy of the next few years. The Greeks tended to interpret the word as applying to the Greeks of Greece proper; the Romans, at least at times, tended to give it a wider interpretation. In the dialogue between Philip and Aemilius at Aby-

<sup>43</sup> To illustrate, Bickermann (p. 173 [35]), who holds that Lepidus had not been instructed to present a declaration of war to Philip at Abydos, argues that, if the statement made to Nicanor was the *clarigatio*, then the statement of Lepidus cannot have been an *indictio belli* for the reason that it contained more clauses than the statement made to Nicanor, while an *indictio belli* must always have the same scope as the *clarigatio*. Passerini (pp. 278-79), on the other hand, considers the ultimatum presented to Nicanor as well as the one presented to Philip an *indictio belli*. This he considers proved by Livy xxxi. 8. According to this passage, when the senate consulted the fetials, it consulted them exclusively on the point whether the declaration of war had to be made to Philip in person or not. This, of course, implies that the senate already had resolved to go to war. Thus, if Passerini is right, the theory that Rome intervened without desiring war falls to the ground.

<sup>44</sup> Pp. 76-78 (18-20).

thus (as reported in Polyb. xvi. 34) it is implied that it covered not only Athens but also Cius and Abydus, in other words, not only Greeks of Greece but also other Greek cities. On the other hand, the account given by Polybius of the conference at Nicaea and the subsequent proceedings before the senate implies that the term "Hellenes" in this case was understood by the Greeks to apply to the Greeks of Greece proper.<sup>45</sup> Probably the latter definition at first was the normal one, but the looseness of the term made it possible for the Romans to broaden its meaning when they found it advantageous to do so. In the *senatus consultum* which served as a basis for the organization of Greece after the war the clause concerning the freedom of the Greeks was made applicable to Greeks both of Asia and of Europe.<sup>46</sup>

The circumstances under which the Romans presented their ultimatum to Nicanor made the demand that Philip abstain from war against the Greeks seem particularly dictatorial. Whatever one may think of the merits of the quarrel between Philip and the Athenians, it remains true that the latter recently had declared war against the king and had done so at the instigation of two states, Pergamum and Rhodes, that already were involved in hostilities with him.<sup>47</sup> Under the circumstances the demand that Philip abstain from war against the Greeks amounted to a demand that he accept Rome as arbiter of his policy in Greece. The demand was not based on the Peace of Phoenice and was couched in general and indefinite terms without any suggestion of arbitration. This means that an acceptance by Philip of the Roman demands would have meant an abject surrender. The ultimatum presented at Abydus was still more unacceptable, not so much because more specific demands were included as because Philip was given to understand that the prohibition against making war on the Greeks covered not only Athens but also such cities as Cius and Abydus. It

<sup>45</sup> Polyb. xviii. 1-11. Note particularly the statements made before the senate by the Greek ambassadors. They emphasize the importance of the fetters of Hellas (Chalcis, Corinth, and Demetrias), which endanger the freedom of the Hellenes and which, in the interest of this freedom, Philip must be compelled to give up.

<sup>46</sup> Polyb. xviii. 44. 2; Livy xxxiii. 30. 2; for a discussion of the nature of the document see *Class. Phil.*, XXXI (1936), 342-48.

<sup>47</sup> Though we do not possess the continuous narrative of Polybius of these events and though there is a break between xvi. 26 and 27, it is certain that the declaration of war preceded the Nicanor incident. If, when the Athenians declared war, the Romans already had interceded with Nicanor, there would have been no need for Attalus to assure them that they could count on the aid of the Romans.

is no wonder that Philip replied that the Romans ought not to violate the treaty and make war on him but that, if they did, he would defend himself. This remark clearly implies that the relations of Rome and Macedonia were regulated by treaty but that the course pursued by the Romans was a complete violation of the treaty.

The conclusion, therefore, must be that the theory that the Romans, when they intervened in Greece in 200 B.C., at first wished to avoid war and made only the minimum representations demanded by the treaty must be rejected. On the contrary, their demands, when first presented, went beyond the treaty and were couched in terms that made it impossible for Philip to accept them without complete self-abasement. This, in turn, indicates that the Roman statesmen responsible for the embassy were resolved at the outset to have war. This should throw some light on Roman policy at the time, though it leaves many questions unsolved. It does not solve the problem of the general merits of the issue between Rome and Philip. No more does it explain the motives that caused Roman leaders to desire war. Nor does it show whether their policy was due to imperialistic dreams—for the failure to make annexations by no means excludes imperialism—or whether it was due to fear, and, if due to fear, whether it was fear of Antiochus or fear of Philip.<sup>48</sup> The present study has already dealt with too much inflammable material without attempting to wrestle with such further difficulties. Nevertheless it may not be out of place to suggest that there may have been a decided mixture of motives but that the events of the year 200 show that a majority of the senatorial leaders by that time had decided that war was desirable or, at least, necessary.

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<sup>48</sup> The theory of Holleaux that Rome feared Antiochus is too well known to need to be mentioned. Passerini (pp. 542-62) argues that the policy adopted by the Romans was due to the fear of Philip. G. T. Griffith ("An Early Motive of Roman Imperialism," *Cambridge Historical Journal*, V [1935], 1-14) largely accepts Holleaux but argues that also fear caused by the development of the Macedonian fleet must be reckoned with. In my opinion these scholars have given insufficient consideration not only to the Peace of Phoenice but also to the events that preceded the treaty. Sempronius was sent to Greece with ten thousand infantry, one thousand cavalry, and thirty beaked ships—indication that, if the Aetolians were willing to co-operate, the Romans were prepared to conduct operations on a large scale. Furthermore, it should be noted that he is reported to have been given a proconsular imperium, though he had not yet been consul (Livy xxix. 12. 2). This, if correct, indicates that his command was considered important.



## THE CYRENEAN HIKESIOI<sup>1</sup>

HAROLD J. STUKEY

THE great ritual inscription found at Cyrene<sup>2</sup> ends with a section introduced by the title ΙΚΕΣΙΩΝ.<sup>3</sup> This word, like ΑΠΟΛΑΩΝ ΕΡΗΣΕ at the beginning of the inscription, is carved in larger letters than the rest and occupies a line by itself.<sup>4</sup> It is followed by three paragraphs of ritual directions, each with a subtitle, respectively (1) *ικέσιος ἐπακτός*, (2) *ικέσιος ἄτερος, τετελεσμένος ἢ ἀτελής*, and (3) *ικέσιος τρίτος, αὐτοφόνος*. The studies that have appeared hitherto all agree in taking *ικέσιος* as a synonym of *ικέτης*, "suppliant,"<sup>5</sup> and the passage has been accepted as an important contribution to our knowledge of Hellenic laws of asylum.<sup>6</sup> The third paragraph will easily admit of such an interpretation. In the others, however, and particularly in the first, to regard the hikesios as a suppliant creates a number of difficulties. It is therefore possible that a more satisfactory explanation can be found.

The text of the first paragraph follows:

*ικέσιος ἐπακτός· αἱ κα' ἐπιπεμφθῆι ἐπὶ τὰν*  
30 *οἰκίαν, αἱ μέγ κα' ἴσαι ἀφ' ὅτινός οἱ ἐπῆνθε, ὁ-*

<sup>1</sup> I wish here to acknowledge my debt to my teacher, Professor George M. Calhoun of the University of California, who suggested the subject of this paper and has generously helped me with advice and criticism in its preparation.

<sup>2</sup> Silvio Ferri, "La Lex Cathartica di Cirene," *Notiz. Arch.*, IV (1927), 93 ff.; Ulrich von Wilamowitz-Moellendorf, "Heilige Gesetze," *Sitzb. preuss. Akad.*, 1927, pp. 155ff.; Gaetano De Sanctis, "Epigrafica: IX. Le decretali di Cirene," *Riv. fil.*, V (1927), 185 ff.; Ludwig Radermacher, "Heilige Gesetze," *Anz. Akad. Wien*, LXIV (1927), 182 ff.; Kurt Latte, "Ein sakrales Gesetz aus Kyrene," *Arch. Religionswiss.*, XXVI (1928), 41 ff.; Achille Vogliano, "Nuovi studi sulle decretali di Cirene," *Riv. fil.*, VI (1928), 255 ff.; Solmsen-Fraenkel, *Inscriptiones Graecae ad illustrandas dialectos selectae*<sup>4</sup> (1930), No. 39; Gaspare Oliverio, "Cirenaica," *Doc. antichi dell' Africa ital.*, II, No. 1 (1933), 7 ff. These works will be cited henceforth simply by the authors' names.

<sup>3</sup> B 28 ff.

<sup>4</sup> Ferri, Figs. 1 and 3 (following p. 100); Oliverio, Figs. 1 and 3.

<sup>5</sup> This usage is attested by Hesych. and Suid.; s.v. *ικέσιος*. It is approximated, as Wilamowitz notes, in *Soph. Ant.* 1230 and *Eur. Med.* 710.

<sup>6</sup> Ferri, pp. 124 ff.; Wilamowitz, pp. 167 ff.; De Sanctis, pp. 201 ff.; Radermacher, *loc. cit.*; Latte, pp. 47 ff.; Vogliano, pp. 300 ff.; Oliverio, pp. 22 ff., 79 ff. Cf. Rudolf Herzog, "Heilige Gesetze von Kos," *Abh. preuss. Akad.*, 1928, p. 36.



νυμαξεί αὐτὸν προειπὼν τρῖς ἀμέρας, αἱ δὲ  
 κα τεθνάκηι ἔγγαιος ἢ ἀλλῇ πη ἀπολώλη[ι],  
 αἱ μέγ κα ἴσαι τὸ ὄνυμα, ὄνυμαστί προερεῖ, αἱ  
 δὲ κα μὴ ἴσαι, "ὦ ἄνθρωπε, αἶτε ἀνὴρ αἶτε γυνὰ  
 35 ἑσσί," κολοσὸς ποιήσαντα ἔρσενα καὶ θήλεια[ν]  
 ἢ καλῖνος ἢ γαῖνος ὑποδεξάμενον παρτιθ[έ]-  
 μεν τὸ μέρος πάντων. ἐπεὶ δὲ κα ποιήσας τὰ  
 νομιζόμενα, φέροντα ἐς ὕλαν ἀεργὸν ἐρε[ῖ]-  
 σαι τὰς κολοσὸς καὶ τὰ μέρη.<sup>7</sup>

Exegesis of this passage has followed the lines marked out by Wilamowitz and De Sanctis.<sup>8</sup> The former sees in this statute a survival of very ancient social institutions. The hikesios is the son or vassal of a Cyrenean who sends him to another citizen's house, or of a foreigner who sends him to Cyrene to try his luck. It is not essential that he be a fugitive. The rite is the formal transference of power over the suppliant from the old lord to the new. If the householder knows the former lord's name, he need only address him by it whether he is alive or dead. If he does not know the name, he says, "O person, be thou man or woman," and makes two images,<sup>9</sup> male and female, of wood or clay, to which he gives a part of all the goods that the suppliant has brought with him.

In De Sanctis' opinion, on the other hand, the suppliant is always a foreigner, fleeing the consequences of some wrongdoing. He is sent by someone who has the right to recommend him. The Cyrenean who receives him must formally satisfy the demands of justice, and, if the

<sup>7</sup> Solmsen-Fraenkel, No. 39, B 29 ff. The only variant readings are l. 31, *τρίς* (Radermacher, Oliverio), and l. 38, *ἐπί[σ]σαι* (Ferri, De Sanctis).

<sup>8</sup> Ferri's view, which is based on the unwarranted assumption that the first and second paragraphs as well as the third deal with homicides, is thoroughly refuted by De Sanctis, pp. 204 ff. When Ferri returns to the question in "Note d'epigrafi cirenaica," *Historia*, III (1929), 399, he merely cites Hdt. i. 35 without answering any of the arguments brought against him.

<sup>9</sup> Wilamowitz supports this rendering of *κολοσός* by other examples, and argues that the word originally had no connotation of hugeness. His conclusions have since been confirmed by the fuller study of E. Benveniste, "Le sens du mot ΚΟΛΟΣΣΟΣ," *Rev. phil.*, VI (1932), 118 ff. Benveniste, however, goes rather beyond the documentary evidence when he restricts the essential significance to "funerary statuette," "image of the dead." It will be seen that the present instance in particular does not support his position.

injured person has died, must protect himself against the peculiar vengeance of the dead. For this reason he calls the injured one by name, summoning him, as it were, to come and state his complaint. Since the summons is naturally not answered, the complaint is held to have failed. If the plaintiff is living and his name is known to the suppliant's host, the proclamation is enough. If, however, the person who has been wronged is dead, the householder fashions the images (one if he knows the person's name and hence his sex) and carries out the other directions. The latter are prescriptions for a ritual banquet, for *ὑποδεξάμενον παρτιθέμεν* have their usual meanings of inviting guests and serving food, and *τὸ μέρος πάντων* is the portion of the food that is due to each one who shares the meal.<sup>10</sup>

It will be seen that after line 30 the *hikesios* is not mentioned. Hence in the two interpretations outlined above, and in any other that sets out from the same premise, we have a law of suppliants that devotes itself almost entirely to ritual intercourse between the suppliant's host and some other person, without making clear the reason for the supplication or the suppliant's position during the time occupied by the ceremony or the fate in store for him at its end.<sup>11</sup> Thus apart from the one word there is nothing in the passage to indicate that it concerns a suppliant. On the other hand, certain features are incompatible with the theory of a suppliant. First, the *hikesios* is not sent merely to the house but to the family, for in this inscription *οἰκία* signifies the household, while the dwelling is *ὄροφος*.<sup>12</sup> Second, the provision for a proclamation to an unknown person would imply that the suppliant is either unwilling or unable to tell what name to use. Since he would have no valid reason for concealing the name of the person who had sent him, Wilamowitz concludes that he must have come without being sent by anyone else. The explanation obviously forces the text. De Sanctis' interpretation avoids the difficulty of the name, for the suppliant might have injured someone unknown to him. However, the conception of the vengeful pursuer is itself open to question,

<sup>10</sup> Oliverio follows De Sanctis. Latte and Vogliano, on the other hand, hold with Wilamowitz that the *hikesios* is a vassal, while Radermacher's position is not quite clear. All three, however, accept from De Sanctis the banquet to the dead.

<sup>11</sup> Cf. Radermacher, p. 185; Vogliano, p. 304.

<sup>12</sup> George M. Calhoun, "Lex sacra Cyrenaica," *Class. Phil.*, XXIX (1934), 346.

for it depends on the separation of *ἐπιπεμφθῆναι* from *ἐπῆνθε*, and a distinction between the person from whom the suppliant comes and the one by whom he is sent.

Now, if the etymological relations of *ικέσιος* be disregarded for the moment, the opening words suggest that the subject of the law is a supernatural visitation rather than a human being. It is perhaps the most common function of *ἐπιπέμπειν* to signify sendings from heaven,<sup>13</sup> and *ἐπέρχεσθαι* also is properly used of such onsets.<sup>14</sup> Even more clearly than these, *ἐπακτός* places the hikesios in the realm of the supernatural. *Ἐπαγωγή* has a well-recognized use as an almost technical term denoting the sending of evils, or, more particularly, the sending of harmful divinities against others by means of magical rites.<sup>15</sup> In two famous passages, *Republic* 364C and *Laws* 933D, Plato includes *ἐπαγωγαί* with incantations and *defixiones* as devices for injuring an enemy. And in words that the inscription before us brings to mind Theophrastus thus describes the Superstitious Man: *καὶ πυκνὰ δὲ τὴν οἰκίαν καθάραι δεινός*, *Ἐκάτης φάσκων ἐπαγωγὴν γεγόνει*.<sup>16</sup> A like connotation of magical action is borne by *ἐπακτός* itself. When Phaedra assures her nurse that her distress is not due to the stain of blood but to a miasma of the mind, the nurse's thoughts turn at once to witchcraft, and she asks, *Μὼν ἐξ ἐπακτοῦ πημονῆς ἐχθρῶν τινος*?<sup>17</sup> Similarly, a Cnidian inscription invokes vengeance on anyone who uses a *φάρμακον ἐπακτόν*, i.e., a charm, against the author.<sup>18</sup>

<sup>13</sup> Examples in Liddell-Scott-Jones, s.v.

<sup>14</sup> Note especially Dem. xxiv. 121: *οὐκ ἀπὸ ταῦτομάτου τὴν ὕβριν καὶ τὴν ὑπερηφανίαν ἐπελθεῖν Ἀνδροτίωνι, ἀλλ' ὑπὸ τῆς θεοῦ ἐπιπεμφθείσαν*.

<sup>15</sup> Tim. *Lex.*, s.v. *ἐπαγωγαί*.

<sup>16</sup> *Char.* xvi. 7. The translation given by Liddell-Scott-Jones, "saying that Hecate has put it under a spell," is misleading. It is more accurate to render with Jebb, "Hecate has been brought into it by spells."

<sup>17</sup> Eur. *Hippol.* 318, with schol. *in loc.*

<sup>18</sup> *GDI* 3545: *μὴ [τύχοι εἰς] λάτῳ, εἴ τι ἡ ἐμοὶ πεποίκει φάρμ[ακον] ἢ ποτόν ἢ κατάχριστον ἢ ἐπακτόν [ἢ τινι] ἡμῶν*. Liddell-Scott-Jones cite *ἐπακτόν* here as a noun with the meaning "charm." Apparently *φάρμακον* . . . *κατάχριστον* is taken as a formulaic unit on the basis of such expressions as Eur. *Hippol.* 516. But *φάρμακον* itself signifies "charm" as well as "drug." It is the "medicine" of the medicineman (cf. Clyde Pharr, *Trans. Amer. Phil. Assoc.*, LXIII [1932], 273). It can be used as a generic term including both specific meanings; cf. Theocr. xi. 1 ff. and the two kinds of *φαρμακία* in Plato *Laws* 932E ff. For these reasons it would seem that *ἐπακτόν* is an adjective co-ordinate with *ποτόν* and *κατάχριστον*, and that the phrase means "medicine" in the form of a potion, a salve, or a charm."

We may conclude, then, that the householder is in fundamentally the same position as the Superstitious Man: He believes that some harmful supernatural power has been invoked upon his household and takes measures to counteract it. But, whereas the one gets rid of his visitation by a purification, the other employs a different method, directing his efforts to winning over his enemy. From this point of view the householder's knowledge or ignorance of the sender's name no longer presents a difficulty. He "knows" from whom the *hikesios* comes if he feels justified in holding a particular person responsible for his trouble. If he is not sure whom to blame, he does not "know" the sender. In the former case he calls the supposed offender by name; in the latter, he uses the indefinite address. Whether the sender is alive or dead makes no difference.<sup>19</sup>

After De Sanctis' elucidation of B 36 and 37 there can be little doubt that the rite of the images is a banquet at which the images represent the sender of the *hikesios*. From what we have just seen, however, it cannot be primarily or essentially a rite for the dead. If there is any restriction in its use, so that it is not employed in all cases of invoked visitations, we must hold with Wilamowitz that the occasion for its performance is ignorance of the sender's name, for the householder is bidden to make two figures, whereas he would need only one for a banquet to a known person. But such a position is hardly tenable if the law is viewed as a whole.<sup>20</sup> When the sender is unknown, the proclamation and banquet are mutually complementary; both are necessary to the success of the householder's purpose. As far as the evidence goes, the entire proclamation is comprehended in the words "O person, be thou man or woman," which serve as an invitation to the feast. The address to the known sender seems to be equally simple, consisting in the bare pronouncement of the name; there is nothing to indicate that it has a broader purpose than the other. Thus it is most probable that a banquet was indispensable in this case also, but it was left to the householder's common sense to make the slight alteration that would be necessary in the procedure. In the context

<sup>19</sup> This interpretation of the conditions in B 30 ff., which is Wilamowitz', is the only one possible on the present theory, for the householder could not know that the sender was dead unless he knew who he was.

<sup>20</sup> Cf. De Sanctis, p. 204.

the intention of the law would be clear for all the brachylogy of its language. The absence of explicit directions would create no more doubt or confusion than, say, the omission of an expressed subject to ἵσαι in B 30.

As regards the form of the banquet, we may suppose that it was a common meal of which the householder partook along with his guest or guests. It is, therefore, to be compared not only with feasts for the dead but with communion sacraments in general. The principle on which it is supposed to operate is found whenever one man shares his board with another; eating of the same food creates a bond between host and guest.<sup>21</sup> In primitive thought the two are believed to be joined by a sort of physical sympathy.<sup>22</sup> However, from Homer onward, the civilized Hellene believed that it was a moral law that compelled the one to help, favor, and protect the other.<sup>23</sup> Which of the two conceptions would play the greater part in the mind of a Cyrenean who had occasion to use the ceremony it is impossible to determine. The point is really immaterial, for either would give him the satisfaction of feeling that he had appeased his enemy or, at least, that he had conquered him and prevented him from doing further harm.

After the banquet the householder still has one thing left to do. Owing to the mutilation of B 38 his procedure is not immediately clear. Ferri and De Sanctis read ἐπέ[σ]σαι, *oscillare*, and take ὕλη ἀεργός to be a barren tree, *infelix arbor*. However, since we are dealing neither with the worship of the dead nor with fertility rites, there is no call for an oscillation here.<sup>24</sup> Accordingly, Wilamowitz' restoration of ἐπέ[ι]σαι is to be preferred, with Radermacher's translation, *niederlegen*.<sup>25</sup> What the celebrant does, therefore, is to take the images and food to an untilled wood and leave them there. In other words,

<sup>21</sup> Gruppe, *Griech. Myth. u. Religionsgesch.*, p. 729; Salomon Reinach, *Cultes, mythes et religions*, II, 43; Pfister in Pauly-Wissowa, XI, 2173, s.v. "Kultus."

<sup>22</sup> Cf. Frazer, *Golden Bough*<sup>3</sup>, III, 130.

<sup>23</sup> The Homeric view is expressed with particular clarity in *Od.* xiv. 401 ff.

<sup>24</sup> For the meaning of swinging-rites cf. Daremberg-Saglio, *Dictionnaire*, s.v. *Αλώπα*, and Farnell, *Hero-Cults*, pp. 31 ff.

<sup>25</sup> For ἐπέθειν in the sense of "deposit" in A 8-9 of this inscription see Maas, *apud* Vogliano, p. 272, and cf. schol. in Theocr. v. 24. In a later note (*Historia*, III [1929], p. 399), Ferri suggests that if ἐπέσαι is right it should mean "bury." However, Livy viii. 10. 12, on which he bases his conjecture, has little in common with the present situation.

the ceremony proper—the effective part of the action—ends with the banquet, and what follows is merely the disposal of the properties.<sup>26</sup> The householder does not want to keep them about, for they are uncanny, taboo, or sacred. Because of the magical bond created by the rite he may not dispose of them as common rubbish without injuring both himself and the man he wishes to conciliate. To leave them where others can find them is dangerous. Taking them to the wood gets them safely out of the way.<sup>27</sup>

This first paragraph, then, presents the hikesios as a visitation or divinity that works harm. In order to refine and clarify this conception it will be convenient to omit the second paragraph for the present and pass directly to the third. The text is Solmsen-Fraenkel's.

- 50 *ικέσιος τρίτος, αὐτοφόνος· ἀφικετεύεν ἐς [τρι]-  
πολίαν καὶ τριφυλίαν. ὥς δὲ κα καταγγήλε[ι] ἰκέ[-  
σθαι, ἴσσαντα ἐπὶ τῷ ὠδῷ ἐπὶ νάκει λευκ[ῶ]ι νί[-  
ξεν καὶ χρῖσαι καὶ ἐξίμεν ἐς τὰν δαμοσ[ίαν]  
ὁδόν, καὶ σιγὴν πάντας ἥ κα ἔξοι ἔωντι[ . . . .*
- 55 *ἀ]ποδεκομένους τὸν προαγγελτῇ[ρα. ἐς ἰα-  
ρό]ν παρίμεν τὸν ἀφικετευό[μ]εν[ον] . . . .  
θε[ῶ]ν καὶ τὸς ἐπομένους [ἀναμένεν ἔξοι· ὁ  
δὲ θ]υσεῖ θύη καὶ ἀλ . . . .  
αἰ] δὲ μή. . . . .<sup>28</sup>*

<sup>26</sup> This explanation is Radermacher's, slightly modified, inasmuch as I should derive the supernatural quality of the things from their employment in the rite instead of from their association with the dead.

Several interesting points of comparison with the Cyrenean rite are offered by *Pap. Mag. Gr.*, I, 86 ff. The passage gives the directions for securing a familiar spirit (*πάρεδρος*). When the spirit finally comes, the celebrant seats him before a loaded table and shares the food and drink with him. Here, too, parts of the food remain to be disposed of after the feast; in this case, however, they can be sacrificed to the spirit after this departure, for he is a god or an angel.

<sup>27</sup> There may also have been a feeling that the unworked wood, even if it were no more than a stand of brush in the midst of cultivated fields, comprised a part of the world that lay outside the boundaries of men's habitations. Then placing the images and food there would be tantamount magically to putting them out of the world entirely, more or less analogous to setting them outside of a magic circle.

<sup>28</sup> While variations from this text are numerous, the one only that bears upon the present argument is Oliverio's restoration of B 55 ff.: [. . . ἐπὶ ἰα | ρῶ]ν παρίμεν τὸν ἀφικετευ[ό]με[ν]ον τῶν δαμο | τελ]έων καὶ τὸς ἐπομένους [βοηθὲν τὰ δίκαια· ὁ | δὲ θ]υσεῖ θύη καὶ ἄλλ[α] δωροτελεσεῖ τῷ ἀρχαγέτ | αι θεῷ, αἰ] δὲ μή [—].



The ceremony is provided for the benefit of a manslayer.<sup>29</sup> As far as the procedure can be made out it seems to be as follows: Someone is to intercede for the slayer<sup>30</sup> at the Tripolia and Triphylia, which is the seat of the magistrates of the three tribes that make up the state.<sup>31</sup> There he makes an announcement, and then seats the slayer on the threshold on a white fleece, washes him, and anoints him. Next they go out into the public street, preceded by a herald who gives warning of their coming.<sup>32</sup> As long as they are outside, the members of the slayer's party keep silence. After some further action a sacrifice is offered.

With the supposition that the hikesios is a visitation for homicide<sup>33</sup> or an avenger, the rite is a device of aversion or propitiation. Hence it belongs to the second of the two classes of practices that Rohde<sup>34</sup> distinguishes in the religious treatment of homicide. The slayer must reckon not only with the pollution of blood, which is removed by purification, but also with the wrath of his victim and of the gods who will avenge him. To avert this danger a propitiatory sacrifice must be offered. Thus, after Circe<sup>35</sup> has cleansed Jason and Medea with the blood of a pig, she makes an offering to Zeus that he may become propitious and stay the dread wrath of the Erinyes.<sup>36</sup> Now, although

<sup>29</sup> Since this is the first and perhaps the only article that dealt with homicide, *αὐτοφόνος* here may well have the general meaning assigned to it by the editors, although classical authors use the word only of homicide within the family.

<sup>30</sup> The presence of both active and middle-passive forms of *ἀφικερέειν* favors this interpretation as against any that makes the slayer the subject of the command (cf. Wilamowitz, p. 171, and Latte, p. 48).

<sup>31</sup> Cf. De Sanctis, pp. 207 f.

<sup>32</sup> Latte, p. 49; cf. Wilamowitz, p. 171.

<sup>33</sup> For this use of *αὐτοφόνος* cf. Aesch. *Ag.* 1091, *Seven* 850.

<sup>34</sup> *Psyche*<sup>6</sup>, I, 271 ff.

<sup>35</sup> *Ap. Rh.* iv. 698 ff.; cf. the sacrifice and purification required of an Athenian manslayer on his return from exile (*Dem.* xxiii. 72 and 73).

<sup>36</sup> It is only the sacrifice that affords a comparison with the rite of the hikesios. The purifications differ in form and have different purposes. Our inscription does not appear to require the use of blood; yet the belief that blood was necessary to wash away the stain of blood was so strong that the doctrine of impurity from bloodshed can hardly have been involved when blood was not used in the cleansing. Hence the Cyrenean purification is probably not an essentially independent ceremony co-ordinate with the sacrifice, as Circe's seems to be, but an indispensable preparation for the offering.

It follows that Oliverio's text of B 55 ff. (n. 28) is not acceptable since it denies a special sacrifice for the slayer's benefit.

Zeus is here the patron of suppliants—Zeus Hikesios—our hikesios is more closely akin to the Erinyes. First, unless he differs greatly from the first hikesios, and, as we shall see, from the second also, he is not the god to whom the slayer appeals but a subordinate agent of punishment. Second, the hikesios is the double in name and function of certain Attic avengers of homicide, the *προστρόπαιοι* of the slain of whom Antiphon<sup>37</sup> makes so much. For as a common adjective *προστρόπαιος* is synonymous with *ικέσιος*; as a divine appellative it signifies the god to whom one turns in entreaty.<sup>38</sup> *Προστρόπαιοι*, in turn, are closely related to *ἀλιτήριοι*, *παλαμναῖοι*, and *ἀραῖοι*.<sup>39</sup> They differ from one another only as the causes that set them in action are variously regarded as pleas for vengeance, sin, murder, or curses. Judged by their works, they are all alike and all similar to *alastores* and Erinyes.<sup>40</sup> We may conclude, therefore, that hikesioi are but a branch of a large and well-known family.

It is now possible to find a place for the second hikesios, whose origin and functions are not expressly stated. I reproduce Oliverio's text:

- 40 *ικέσιος ἄτερος, τετελεσμένος ἢ ἀτελής· ἰσ-*  
*σάμενος ἐπὶ τῷ δαμοσίῳ ἰα(ρ)ῶνι, αἱ μέγα καὶ προ[φέ]-*  
*ρηται, ὁπόσσω καὶ προφέρηται, οὕτως τελίσκ[ε]-*  
*σθαι. αἱ δὲ καὶ μὴ προφέρηται, γὰρ καρπὸν θ[ύ]-*  
*εν καὶ σπονδὰν καθ' ἔτος ἀεί. αἱ δὲ καὶ παρῇ, ἐ[ς]*
- 45 *νέω δις τόσσα. αἱ δὲ καὶ διαλίπηι τέκνον ἐπι[λα-*  
*θόμενον καὶ οἱ προφέρηται, ὅ τι καὶ οἱ μαντε[υ]-*  
*ομένῳ ἀναιρεθῇ, τοῦτο ἀποτεισεῖ τῷ θεῶ<ι> κ[αὶ]*  
*θυσεῖ, αἱ μέγα ἴσαι, ἐπὶ τῷ πατρῶϊον, αἱ δὲ μὴ, [χρή]-*  
*σασθαι.*<sup>41</sup>

Apart from the title this section is fairly straightforward. The person to whom the law applies must seat himself at the public shrine to

<sup>37</sup> iv. 1. 4, 2. 8; cf. ii. 3. 10, iii. 4. 9.

<sup>38</sup> Eust. 1807. 11.

<sup>39</sup> For examples cf. Liddell-Scott-Jones, s.vv. The use of these words in reference to living men does not concern us here.

<sup>40</sup> The Erinyes are frequently curse deities, as Gruppe notes (*op. cit.*, p. 767). So are the others, according to Poll. v. 131; cf. further Rohde, *op. cit.*, I, 275 ff.

<sup>41</sup> With one exception the variant readings of other recensions do not affect our problem. Vogliano and Solmsen-Fraenkel, however, beg the question of the hikesios' identity by their punctuation, placing a comma after *ἀτελής* and a full stop after *ἰαρῶν*.

consult the oracle. If he gets a response, he must pay whatever the oracle commands. If he does not get an answer, he must offer produce of the earth and a libation yearly forever. The duty of making this offering passes from father to son. If the one who originally incurred it fails to fulfil it, he need only double the amount of the offering the following year.<sup>42</sup> However, if one of his descendants omits the offering through forgetfulness, he must consult the oracle and pay whatever it orders as a penalty to the god, and in addition must sacrifice at his ancestral tomb if he knows where it is.

Now the first two lines of the paragraph read most naturally if the hikesios is the man who goes to the temple for advice. Nevertheless, we are under no compulsion to construe the title so closely with the following words. This inscription changes subjects rapidly and without warning. In particular, the headings of sections are likely to be independent of the actual statute. We have seen that in B 50 the hikesios cannot be the subject of the first command; in A 8 the words *κᾶλον ἐν ἱερῶι πεφυκός* likewise stand by themselves. In the present instance, then, we are at liberty to treat the heading as an independent and self-contained unit whose purpose is only to designate the occasion of the ceremony. There is consequently no obstacle to regarding the hikesios as a noxious divinity like the others. His functions may be inferred from those of the Erinyes and their likes. As the third hikesios takes their place in the punishment of homicide, so the second in all likelihood serves in their stead on other occasions. Hence he doubtless first of all guards the interests of parents and other blood-relatives. On the other hand, he seemingly does not confine his protection to this small circle, any more than do Erinyes and *alastores*.<sup>43</sup> The son's oblation at his father's grave simply atones for his neglect of the hereditary rites and implies nothing as to the cause of the original visitation. Thus we may assume that the duties of this hikesios are to avenge wrongdoing in general and to give effect to curses. Now in the *Seven against Thebes*,<sup>44</sup> when Eteocles declares that his father's curse is driving him to fight with his brother, the chorus urge

<sup>42</sup> For ἐς νέω cf. Paul Maas, *Riv. fil.*, VI (1928), 413 f.

<sup>43</sup> Erinyes punish perjury (*Il.* xix. 259) and protect beggars (*Od.* xvii. 475). *Alastores* having no evident source in bloodshed or wrongs to kin are found frequently in the tragedians, e.g., Aesch. *Sup.* 415, *Pers.* 354; Soph. *Trach.* 1235; Eur. *Hippol.* 820.

<sup>44</sup> 695 ff.

him to resist the impulse and remind him that the Erinyes will depart from the house when the gods accept sacrifice. This belief corresponds exactly to practice at Cyrene. The god of the oracle shows an abiding interest in the annual offering of grain and wine that clearly marks him, and not the hikesios, as the one to whom it is regularly made.

Since heaven is notoriously slow to punish sin, men can hope to escape its wrath altogether as well as to win relief when once it falls upon them. In this fact, I believe, lies the clue to the title-phrase *τετελεσμένος ἢ ἀτελής*.<sup>45</sup> Whereas the headings of the other sections indicate the cause of the hikesios' attack, this one refers instead to its consummation. If the hikesios were an impersonal visitation, the phrase would mean "brought to pass or not brought to pass." Since he is undoubtedly personal, *ἀτελής* is rather "unsuccessful, not having accomplished his purpose," and *τετελεσμένος* consequently has the sense "successful, perfect in function."<sup>46</sup> The title, then, makes it clear that the rite is equally efficacious as prophylactic or as cure. Without such an assurance many a man who expected a visitation, but had not yet felt its effects, would have feared that this help was not for him. The lawgiver carefully forestalls any misconception of his purpose, just as he leaves no doubt that the sender of the first hikesios is to be addressed by name, if possible, even though he is dead.

We have shown that the conception of the hikesios that we derived from the opening words of our text is consistent with the later parts of the inscription and with evidence from other sources. We have found that hikesioi are evil divinities of a familiar kind, to be numbered among the gods whose offices, in the words of Isocrates,<sup>47</sup> are calamities and punishments, who are honored neither in our prayers nor in our sacrifices, but are met by us with attempts to drive them

<sup>45</sup> No exposition of the hikesios as a suppliant satisfactorily accounts for these words. For the most part they are taken to mean "initiated or uninitiated." But if the hikesios comes to the temple to be initiated, as Wilamowitz holds, it is hard to see why any other initiation should be mentioned at all, or, if mentioned, why it should not be more fully designated. If, with De Sanctis and Vogliano, we make the hikesios sue for asylum in the temple or for the god's protection in general, reference to an initiation is even more out of place. Ferri's conjecture that the suppliant is "unclean or excommunicated" depends on the obscure definitions of Hesych. *s.v.* *τετελεσμένος* and Suid. *s.v.* *ἐτελέσθησαν*.

<sup>46</sup> If this analysis is correct, the Thera dialect uses *τελεῖν* where Attic and Ionic prose would have *τελεῖν* (cf. Liddell-Scott-Jones, *s.vv.*).

<sup>47</sup> v. 117.

away. It is somewhat startling at first to find these malign beings bearing the same name as the gods of suppliants. Our impression of a suppliant's god is likely to be a favorable one. We see him mostly from the suppliant's point of view, helping him in his need and protecting his weakness. As it happens, moreover, most petitioners of *ικέσται θεοί* that we meet seek such favors for themselves as do not at the same time harm others. The Cyrenean hikesioi, on the other hand, deal only in the more vicious kinds of supplication, and they appear to us as they appeared to the unfortunate objects of their attacks.

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## ANGIPORTUM, PLATEA, AND VICUS

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THERE is great confusion among modern editors and translators concerning the various Greek and Latin words meaning "street." The present paper attempts to determine the precise meaning of the ordinary terms in Latin, though frequent reference is made to the corresponding Greek words.<sup>1</sup>

### I

*Angiportum* is usually translated "alley" in English, but such a translation seems altogether misleading in some passages to be considered later, and one may recall that ancient cities did not, in general, have alleys in the current sense of that term as used in America.<sup>2</sup> The vast majority of streets in Athens and Rome, however, were very narrow.<sup>3</sup>

<sup>1</sup> I wish to thank Professor Prescott for reading this paper and offering numerous helpful suggestions, and Professor W. A. Oldfather and Dr. J. L. Catterall for data from the unpublished index to Cicero's letters.

In this paper the following works are cited merely by the names of the authors: C. O. Dalman, *De aedibus scaenicis comoediae novae*, "Klass.-phil. Studien," Heft 3 (Leipzig: Harrassowitz, 1929); H. Jordan, *Topographie der Stadt Rom im Alterthum* (Berlin: Weidmann, 1871 and 1878); W. Judeich, *Topographie von Athen* (Munich: Beck, 1931); S. B. Platner and T. Ashby, *A Topographical Dictionary of Ancient Rome* (Oxford University Press, 1929); F. Preisigke, *Wörterbuch der gr. Papyrusurkunden* . . . . (Berlin, 1925-29); J. Zeller, "Vicus, Platea, Platiodanni," *Archiv f. lat. Lex. u. Gram.*, XIV (1906), 301-16. Well-known commentaries are also cited merely by the names of their authors.

<sup>2</sup> At Pompeii occasionally a house covers a complete square block (*Casa del Fauno*, Region VI, block xii, house 2), and often houses run through the block, having their main entrances on one street and rear entrances on another. Houses situated on a corner often have two entrances (IX, v, 18-21; *Casa del Menandro*, I, x, 4). One may note, however, that in Region VI, blocks vii-x, the houses with few exceptions have their front entrances on the Strada di Mercurio and often have rear entrances on the parallel streets. These two parallel streets (next to the Strada di Mercurio on the east and on the west) have very few houses facing upon them and sometimes have no sidewalks, thus approaching alleys in their function but still being as wide as many other streets, though not so wide as the Strada di Mercurio. The city of Olynthus, laid out in the second half of the fifth century B.C., regularly had gutters down the middle of each block (the blocks are rectangular). The width of these gutters would have allowed the passage of people on foot, but they were usually blocked by walls (cf. D. M. Robinson, *American Journal of Archaeology*, Vol. XXXVI [1932], Fig. 1, p. 118).

<sup>3</sup> According to the laws of the Twelve Tables, a *via* must be eight feet *in directo*, sixteen feet *in fracto*; cf. Varro *LL* vii. 15; Gaius *Dig.* viii. 3. 8. The Augustan pavement [CLASSICAL PHILOLOGY, XXXII, January, 1937]



The basic meaning of *angiportum* is equivalent to that of the Greek *στενωπός* when used of a thoroughfare although, in translation, *angiportum* might well have been used for various other Greek words, such as *ρύμη*, *λαύρα*, *ἄμφοδον*, etc. In Greek New Comedy, as Dalman has shown, the thoroughfare upon which the characters stand is termed sometimes *πλατεία*, sometimes *στενωπός*, but the latter word seems to have lost its etymological meaning.<sup>4</sup> If *angiportum* is precisely equivalent to *στενωπός* and was used to translate it, as is doubtless the case,<sup>5</sup> then perhaps *angiportum*, too, may have lost its etymological force and

of the Sacra Via is five meters wide in one section (cf. Platner-Ashby, p. 450); cf. Cicero (*De leg. agr.* ii. 96 [text emended]): "Romam . . . non optimis viis, angustissimis semitis. . . ."

<sup>4</sup> Dalman, pp. 66-70. The words *στενωπός* (cf. Plutarch *Praec. ger. reip.* 15 [II 811 B]) and *πλατεία* (Plutarch *Dio* 46 [I 978 C]) both became general terms for streets; cf. Hesychius: *Στενωπός*: ἡ ἀγία, καὶ πλατεία, καὶ ἄμφοδος. The same development is noticeable in *λαύραι* (cf. Herodotus i. 180; Herondas i. 13) and *ἄμφοδοι* (cf. Harmodius of Lepreum, *FHG*, IV, 411 = Athen. 149 C, Hesychius, and Suidas). Judeich (p. 178) says that *στενωπός* denoted *die Masse der kleinen, engen, krummen Gassen* and that *ἄμφοδοι* denoted not thoroughfares but blind *Gässchen*. But he draws distinctions that are too fine, just as the writers on Roman topography are inclined to do (cf. n. 14). The proper terms for blind streets are *τυφλή ὁδός* (cf. Hesychius, *τυφλή ῥύμη* (*Pap. Oxyrh.*, I, 99, l. 9), etc. The word *στενωπός* is not found in the Septuagint or in the New Testament, and it is not listed by Preisigke (but cf. *ῥύμη στενή*), although it is used by certain late authors (cf. Sophocles *Lexicon*). *Πλατεία* and *ῥύμη* seem to have been the usual words in ordinary speech in later Greek (cf. Pollux ix. 38), though the other words are also common in the papyri (except *στενωπός*). How loosely terms for "street" were used may be observed from passages wherein they are interchanged; cf. Herodotus i. 180 (*λαύραι* and *ὁδοί*), Clearchus, *FHG*, II, 310 = Athen. 540 F (*λαύρα* and *στενωπή*—text emended); *Pap. Oxyrh.*, I, 99 (*λαύρα* and *ῥύμη*). In Oxyrhynchus the same proper names (e.g., Temgenouthis) are used now with *ἄμφοδον*, now with *λαύρα* (cf. Preisigke).

<sup>5</sup> *Angiportum* is frequently glossed *στενωπός*. The gender of *στενωπός* and of *ἄμφοδον* varies as does that of *angiportum*. The narrowness of *angiporta* is evident in many passages in the commentators, etc.; cf. Varro *LL* v. 145; vi. 41; Festus 16 (Lindsay [Teubner] = *Glos. Lat.*, IV, 112); *Corp. glos.*, V, 439, 57 (*Glos. Lat.*, V, 176, 983, from Festus according to Lindsay on Festus, *Glos. Lat.*, IV, 112); Donatus on Terence *Ad.* 578; Eugraphius on Terence *Eu.* 845, *Ad.* 578; *Corp. glos.*, II, xii: "CIMITERIVM, locus sepulchorum [sic]. Urbs omnis dividitur in sex partes. Id est templa, domos, vicos, insulas, plateas, et angiportas. . . . Vici sunt publicae constructiones mansionum. Insulae, qui inter vicos sunt horti. Plateae, viae latae a porta in portam. Angiportas (-as *pro* -us, *corr.*) viae angustae inter minores vicos quae exitum ad muros aut nullum aut angustum habent."

On *angiportum* in general cf. *Thes. L. L.* (add Plautus *Pseud.* 1235; *Not. Const. reg.* II; Donatus on Ter. *Eu.* 845; Eugraphius on Ter. *Ad.* 578, *Eu.* 845); Mau in Pauly-Wissowa, I, 2190-91; Landgraf, *Archiv f. L. L.*, V (1888), 139-40, 191. In this paper the singular of the word is given as *angiportum*, the plural as *angiporta*, merely for convenience in reading. There is constant variation in the forms employed and sometimes in the MSS readings; cf. Priscian (Keil, II, 262; cf. suppl. 127, 30).

become a general word for street, especially in translation Latin. *Angiportum* in at least one passage in comedy is clearly used of the thoroughfare upon which the characters stand (Plautus *Pseud.* 960-61):

SIMIA. Habui numerum sedulo: hoc est sextum a porta proximum  
angiportum, in id angiportum me deuorti iusserat. . . .<sup>6</sup>

Here, then, *angiportum* does not refer to an alley, but to a normal street upon which houses have their main entrances. Such usage is not uncommon elsewhere. Cicero says (*Pro Mil.* 64): ". . . nullum in urbe vicum, nullum angiportum esse dicebant, in quo non Miloni conducta esset domus. . . ." Here *vicus* and *angiportum* are certainly inclusive terms. *Vicus* may possibly be interpreted as precinct, and in this case *angiportum* would take in all streets; but perhaps *vicus* here refers to the more important streets (cf. Vitruvius i. 6. 8, quoted below), *angiportum* to the less important ones. In either case the meaning of alley for *angiportum* is out of the question.

Similar passages, in which the house of someone is said to be located on an *angiportum*, are found in *Rhet. Her.* iv. 64, Apuleius (*Met.* i. 21, house of the wealthy but miserly Milo), and Festus (142 Lindsay [Teubner] = *Glos. Lat.*, IV, 272 [text restored]). It is true that, in Roman comedy, an *angiportum* is sometimes a more secretive place than the stage itself (cf. *As.* 741; *Persa* 444, 678) and is apparently thought of as running behind the houses portrayed on stage. Interesting is the following passage from the *Eunuchus* (844-46):

. . . ubi vidi, ego me in pedes quantum queo  
in angiportum quoddam desertum, inde item  
in aliud, inde in aliud: . . .

It is a mistake, of course, to infer from this passage that *angiporta* are necessarily secretive places. Here they are ordinary streets—obviously not blind streets, and the idea of secretiveness is carried by the adjective *desertum*. In the following passage, also, an *angiportum* cannot be a cul-de-sac. This passage is a fragment from a speech of C. Titius (delivered 161 B.C.), in which he describes the life of certain men, now on their way to the *Comitium* (Macrobius *Sat.* iii. 16. 15): "Dum eunt,

<sup>6</sup> Cf. *Pseud.* 971. Possibly *Pseud.* 1235 and Terence *Phor.* 891-92 are parallel (cf. Dalman, pp. 73-74).

nulla est in angiporto amphora quam non impleant, quippe qui vesicam plenam vini habeant." It is well known that jars were placed in the streets by the fullers, who employed urine as a cleaning medium, and one must not suppose that these jars were placed in inconspicuous places.<sup>7</sup> Such conveniences in certain modern European cities, though sometimes placed in alleys, are often (especially in Paris) located on the most frequented boulevards. It is certain that, in this passage of Titius, *angiportum* refers to the thoroughfare, no doubt an important one, upon which the men were making their way to the *Comitium*. The meaning of alley is quite impossible. It is likewise impossible in the following passage from Cicero (*In Ver.* ii. 2. 141): "... ut omnibus in angiportis praedonis improbissimi statua ponatur, qua vix tuto transiri posse videatur." Certainly statues were placed in frequented thoroughfares, and, if they might threaten the passer-by, it would be not because the streets were so narrow that the statues would encroach dangerously upon them, but because even the statue of this robber Verres would seem to threaten the populace. Here the phrase *omnibus in angiportis* embraces all streets of whatever nature.

Passing now to the poets, we find in Catullus (lviii. 4-5):

nunc in quadriuiis et angiportis  
glubit magnanimi Remi nepotes.

One compares Horace (*Carm.* i. 25. 9-10):

invicem moechos anus arrogantis  
flebis in solo levis angiportu, . . .

In regard to the lines from Catullus, Baehrens thinks that a contrast between frequented places and deserted places is intended. But such a contrast would have no point here. The shame and disgrace are heightened by the fact that the woman practices her arts so publicly. The phrase in *quadriuiis et angiportis* seems to include all public places and all streets. Thus *angiportis* is here used as in the foregoing passage from Cicero. Kroll cites the rare Greek word *σποδησιλαύρα* (Hesychius, Eustathius 1921. 58; 1033. 62; 1082. 41), where *-λαύρα* is obviously a general term for street. One may compare Aristophanes *Eccl.* 693-96.

<sup>7</sup> Cf. Martial vi. 93. 1-2; xii. 48. 8 (note that *via* is here used in both passages); and also H. Blümner, *Tech. und Term. der Gewerbe und Künste bei Griechen und Römern* (Teubner, 1912), I, 175-76.

In the lines from Horace, also, *angiportu* seems to be a general term for street. The adjective *solo* is noteworthy, although it is probably a transferred epithet—the street is lonely for the despised old woman (cf. Norden on Vergil *Aen.* vi. 268).<sup>8</sup> Apuleius uses *angiporta* (*Socr.* xix [165]) as a general word for streets, and he uses the singular apparently for a normal street also,<sup>9</sup> although once (*Met.* ix. 2) a mad dog enters a house *de proximo angiportu . . . per posticam*.

In Rome there is no instance of *angiportum* used with a proper name; but two *angiporta* of Cales were named after temples (*CIL*, X, 4650, 4660), which were presumably located on these *angiporta*.

It is remarkable that *angiportum* was still current in the fifth century, when it was used in the Codex Iustinianus as a generic term for "street" (viii. 11. 20, Edict of Theod. and Val., A.D. 439): "Qui sine auctoritate . . . angiportus integros vel partes suis domibus incluserint seu porticus usurparint, procul dubio iura pristina sacratissimae reddere civitati iubemus: . . ." From the same period comes the Catalogue of Constantinople, which is quoted on page 57.

<sup>8</sup> The remark of Vollmer (*Thes. L. L.*, II, 47, s.v. *angiportum*) appears to be misleading in its implications: "apud scriptores subest fere notio secreti, ubi sicarii latrones moechi rem habent." *Angiportum* is frequently glossed *locus secretus*, and beside the passages quoted in the text a passage in Suetonius carries a somewhat similar connotation (*Galba* 10): ". . . per angiportum in balneas transeuntem paene interemerunt." But streets in general had an unsavory reputation; cf. Horace *Epod.* v. 97; Suetonius *Aug.* 45; Apuleius *Met.* ii. 18; Harmodius of Lepreum, *FHG*, IV, 411.

Many modern scholars and some ancient commentators and glosses interpret *angiportum* as a cul-de-sac (Donatus on *Eu.* 845; schol. on Horace *Carm.* i. 25. 10; *Corp. glos.*, IV, 405, 49). Lindsay (commentary on Festus, *Glos. Lat.*, IV, 112) interprets Varro *LL* v. 145 in this manner, but this interpretation is impossible in the opinion of the present writer. The clause *quod exitum non habet ac pervium non est* (on *pervium* cf. Blase, *Archiv f. L. L.*, IV [1887], 322–23) clearly goes with *fundula*, the etymology of which—either according to Varro (*fundus*) or according to modern authorities (cf. Walde-Hofmann, Ernout-Meillet, s.v. *fundus*)—allows this meaning. The etymology of *angiportum*—the true or the false—does not account for such a meaning.

It is clear from many passages quoted above that *angiportum* frequently refers to an ordinary thoroughfare, and even Terence *Adel.* 578, the *locus classicus* for the meaning cul-de-sac, clearly shows upon a careful reading that *angiportum* cannot of itself mean cul-de-sac, but for such a meaning it must be limited by *non . . . pervium*; and without this limitation it is presumed to be a normal street (*Ad.* 576). In *Eunuchus* 845, *angiporta* cannot be "blind streets," regardless of the comment of Donatus on this line. Festus 468 (Lindsay [Teubner] = *Glos. Lat.*, IV, 434) may presume a blind alley as the meaning of *angiportum*, but this passage is not altogether clear.

<sup>9</sup> Apul. *Met.* i. 21; iii. 2; iv. 20; ix. 25. Jordan (I, 523, n. 49a) says that *prima platea* (*Met.* ii. 32) is equivalent to *primus angiportus* (*ibid.* iii. 2), comparing *Met.* ii. 18, 27. But these passages do not seem to refer to the same street.

It now becomes plain that, in Roman comedy, the characters may quite naturally refer to the thoroughfare upon which they stand as an *angiportum*.

## II

Turning now to *platea*, we may again remark that, in Plautus and Terence, the thoroughfare upon which the characters stand is usually called a *platea*.<sup>10</sup> Interesting also are phrases such as *recta platea* (*Cist.* 534) and *qua platea* (*Men.* 881), where *platea* is substituted for the more usual *via*. Catullus uses *platea* once (xv. 6-8):

... non dico a populo—nihil veremur  
istos qui in platea modo huc modo illuc  
in re praetereunt sua occupati. ...

Here *platea* seems to be a general term for "street."

Noteworthy also is a passage in Pliny the Younger (x. 98. 1): "... civitas ... habet ... pulcherrimam eandemque longissimam plateam, cuius a latere per spatium omne porrigitur nomine quidem flumen. ... ." "Boulevard" would be a good translation in this context, preserving in English the foreign flavor which *platea* has in Latin and denoting an important thoroughfare as the passage suggests. Apparently parallel is the following usage in Aelius Spartianus (*Caracalla* 9. 9): "... Viam Novam munivit ... qua pulchrius inter Romanas plateas non facile quicquam invenias." It is possible, however, that *platea* here is merely a generic term for street. At least this generalized meaning seems clear elsewhere in the *Scriptores historiae Augustae*, especially in Aelius Lampridius (*Heliog.* xxxiii. 7): "... et occisus est per scu<ta>rios et per plateas tractus e[s]t sordidissime per cloacas ductus et in Tiberim submissus est." One may compare the following passage in Suetonius, who uses *vicus* as a general term for street but does not employ *platea* at all and uses *angiportum* (*Galba* 10) only once: "... nec ante satiatus est quam membra et artus et viscera hominis tracta per vicos atque ante se congesta vidisset" (*Calig.* 28). In all the *Scriptores historiae Augustae*, *vicus*, meaning "street" or "precinct," occurs only once (*in vico Sulpicio*

<sup>10</sup> Cf. Plautus *Bac.* \*632; *Cap.* 795; *Cas.* 799; *Cur.* 278; *Mil.* 609; *Tri.* 840, 1006; Terence *An.* 796; *Eu.* 344, 1064; *Phor.* 215. *Via* is also used in a general way for the thoroughfare on which the characters stand (cf. *Cas.* 856; *Stich.* 606, etc.). The use of the phrase *ultima platea* is also noteworthy (cf. *Cur.* 278; *Phor.* 215).

[*Heliog.* xvii. 8]), and *angiportum* is not found at all. *Platea* is used as a general term for street not only in the foregoing passage but also in Julius Capitolinus (*Max. et Bal.* ix. 2). Of course *via* is also used as a general term for "street," as it is commonly used in all periods. Apuleius uses *vicus* once (*Met.* iii. 29), but here it means "village." He uses *angiportum* six times and *platea*, which is a general term for "streets," twelve times (cf. *ibid.* ii. 18; iii. 2; iv. 14, etc.).

In general, *platea* is a rare word in classical Latin as the few examples cited clearly indicate. In late Latin, however, it became very popular, as it has been with Plautus and Terence.<sup>11</sup> It does not occur in the letters, speeches, or philosophical writings of Cicero (though it is sometimes restored in *De leg. agr.* ii. 96), or in Petronius, Quintilian, Tacitus, Juvenal, or Martial.

### III

The word *vicus*, cognate with *oikos*, has various meanings, the most common of which is that of "village" (cf. Aetheria vii. 7). It is also applied to a precinct or district of a city and to a street of a city.<sup>12</sup> The Greek word *ἄμφωδον* is similarly used both as a street and as a dis-

<sup>11</sup> Additional cases of the occurrence of *platea* are the following: Plautus *Am.* 1011, *Aul.* 407 (see n. 10); Terence *Adel.* 574, 582 (see n. 10); Horace *Epist.* ii. 2. 70-71; Livy xxv. 25. 8; *Notae Tir. (CNT)* 120. 77 (cf. 78); Aelius Lampridius *Heliog.* xxiv. 6; *Alex.* xxv. 7; Aelius Spartianus *Car.* iv. 2. For a large number of late examples cf. Zeller, p. 306, and the Vulgate. Two *plateae* with proper names are known: the Platea Traiani (Symmachus *Epist.* vi. 37) and the Plateae Antoninianae (Aelius Lampridius *Heliog.* xxiv. 6). The latter should have been included under a separate heading in Platner-Ashby, who mention them under *Palatinus Mons* (pp. 378-79).

*Platea* is glossed *via spatiosa* twice (*Corp. glos.*, IV, 144, 28 = *Glos. Lat.*, III, 68, 8; *Corp. glos.*, IV, 268, 14 = *Glos. Lat.*, V, 100, 7). Cf. n. 4; Isidore *Etym.* xv. 2. 23; *De diff. verb.* 598. The form of the word is explained in Donatus on Terence *An.* 796 and in the grammarians (Keil, VII, 282, l. 31).

On the original Greek word cf. Hesychius *πλατεια*ς ῥύμας, ἀγοραῖς. In the Septuagint the Hebrew word *רחב* or *רחוב*, which is often translated *πλατεια*, is sometimes translated *ρύμη* (e.g., Tob. 13:18). "Piazza" is the common meaning of *πλατεια* in modern Greek, and this is apparently the proper interpretation in many passages in the Septuagint and occasionally in the New Testament; cf. II Chron. 32:6; II Esd. 10:9; Matt. 6:5 (J. M. P. Smith and E. J. Goodspeed, *The Bible: An American Translation* [University of Chicago Press, 1931]). The word used in the Vulgate in these passages is *platea*. One would naturally assume that the meaning "piazza" developed first in Greek, owing to the influence of *πλατὺς*, and that this meaning was then transferred to the Latin word. These passages seem to prove this supposition. The spelling *plātaea* (*CIL*, VIII, 51) indicates that the word continued to be associated closely with its Greek original (cf. Zeller, p. 316).

<sup>12</sup> *Vicus* is frequently glossed *κώμη*, *ρύμη*, and *ἄμφωδον* (*ἄμφωδος*), once *ἐποικ[ε]λτα*; cf. esp. *Corp. glos.*, II, 208, 19 = *Glos. Lat.*, II, 287, 22 and Isidore *Etym.* xv. 2. 12.



trict of a city, being extremely frequent in the papyri when used with a proper name.<sup>13</sup> The word *λαύρα* is also used of both street and district.<sup>13</sup> To distinguish between these two meanings is often difficult or even impossible in a given context. Sometimes the idea in the author's mind was doubtless vague when he used the word.

As for *vicus*, it is usually assumed that a whole quarter or precinct was termed *vicus* (e.g., *Vicus Tuscus*) and, also, that the chief street of that quarter was called by the same name, while the side streets were called *semitae* and the blind streets or alleys *angiporta*.<sup>14</sup> It is doubtless true that the name of a quarter often became applied to the chief street of that quarter (cf. *Στενωπὸς Κολλυτὸς* at Athens [Judeich, p. 180]). This accounts for the common use of *vicus* as an appellative to the apparent exclusion of *angiportum*, at least at Rome, and somewhat at the expense of *via*. But we have already seen that this conception of *angiporta* is incorrect, and the definition of *semitae* here given is equally so.<sup>15</sup> Platner-Ashby name over one hundred *vici*, almost invariably identifying them as streets. But most of these names are taken from inscriptions (e.g., *CIL*, VI, 975) which give only the names of *vicorum magistri* by *vici*. That these *vici* were precincts is clear from Suetonius *Aug.* 30, and few of the names cited by Platner-Ashby can be proved to apply to thoroughfares.<sup>16</sup> It is well known that only two *viae* with proper names are recorded from the republican period (cf. Platner-Ashby, p. 456, s.v. *Sacra Via*), but it would be rash to conclude from this fact that all the other streets were classed as *vici* and so named,

<sup>13</sup> Cf. Preisigke, Band III, Absch. 22; Grenfell-Hunt, commentary on Oxy. pap., II, 242, 12.

<sup>14</sup> So Zeller, p. 301 (cf. Jordan, I, 514).

<sup>15</sup> *Semita* is occasionally used as "street" (cf. Cicero *De leg. agr.* ii. 96), and a street in Rome was called the *Alta Semita*. In Plautus *semita* sometimes means "sidewalk" (*Cur.* 287; *Mer.* 115; *Tri.* 481). *Semita* is used elsewhere in Plautus with its ordinary meaning of "path."

The word *clivus* is also a common word for "street" in Latin; some eighteen names with *clivus* are listed in Platner-Ashby (cf. *Forma urbis Romae*, ed. H. Jordan [Berlin: Weidmann, 1874], 37).

<sup>16</sup> The *Vicus Tuscus* is mentioned in Plautus *Cur.* 482. Dionysius of Halicarnassus (*AR* v. 36. 4) translates the phrase *Τυρρηῶν Οἰκῆσις*, thus clearly considering it a district, not a street (cf. Livy ii. 14. 9; Servius on Verg. *Aen.* v. 560). In Livy xxvii. 37. 15 one may observe how the names of districts and of the streets running through these districts become indistinguishably merged. Several *vici* with proper names are translated by *στενωπὸς*: *Vicus Patricius*, Plutarch *QR* 3 (II 264 C); *Vicus Longus*, Plutarch *De for. R.* 10 (II 323 A); *Vicus Cuprius*, Dionysius *AR* iii. 22. 8.

for doubtless many streets had no official names at Rome—at least in the republican period—just as they had no official names at Athens (Judeich, p. 178). If Rome contained 265 *vici* or precincts (cf. Pliny *NH* iii. 66), a *vicus* could certainly not have been very large, but would have been eleven or twelve acres, approximately equivalent to a modern square block, provided that they were evenly distributed throughout the whole area of the city.

The New Liddell and Scott gives as one meaning of ἀμφοδον, "block of houses surrounded by streets," and, although the passages cited do not appear conclusive for this precise meaning, the formation of the word suggests that this was its original significance (cf. ἀμφιάλος). Festus seems to interpret *vicus* in a similar manner, ascribing to it three meanings: village, block of houses surrounded by streets, and thoroughfare (502 Lindsay [Teubner] = *Glos. Lat.*, IV, 460–61):

... Altero, cum id genus aedificiorum definitur, quae continentia sunt [h]is oppidis, quae . . . itineribus regionibusque distributa inter se distant, nominibusque dissimilibus discriminis causa sunt dispartita. Tertio, cum id genus aedificiorum definitur, quae in oppido privi in suo quisque loco proprio ita aedificata, ut in eo aedificio pervium sit, quo itinere habitatores ad suam quisque habitationem habeant accessum.

Varro, however, seems to make the erroneous assumption that *vicus* originally meant street and introduces a ridiculous etymology (*LL* v. 145 [text emended]):<sup>17</sup> "In oppido vici a via, quod ex utraque parte viae sunt aedificia. Fundula a fundo, quod exitum non habet ac pervium non est. Angiportum, si id angustum, ab agendo et portu." Again, Varro names various *vici* and then remarks (*ibid.* v. 160; cf. v. 8): "Quoniam vicus constat ex domibus. . . ." Doubtless it is mainly from these passages in Varro that editors derive the notion that the basic meaning of the word *vicus* is "row of houses." Of course, the etymology shows that *vicus* in its original meaning consists of houses, but this is true both of a village and of a precinct—two meanings distinct in actual usage.

The two passages in Plautus where *vicus* occurs are ambiguous (*Cur.* 482; *Mer.* 665).<sup>18</sup> Terence does not use *vicus* nor does Catullus or Tibullus. Cicero (in his letters, speeches, and philosophical writings)

<sup>17</sup> Lindsay's interpretation of this passage is impossible; cf. n. 8.

<sup>18</sup> Cf. n. 16; with *Mer.* 665 cf. Suetonius *Claud.* 18.

uses *vicus* meaning "street" or "quarter" twice (*Pro Mil.* 64, already quoted; *De off.* iii. 80; cf. *Epist. ad Att.* 4, 3, 4, 15). The passage from the *De officiis* is interesting: ". . . omnibus vicis statuæ, ad eas tus, cerei." This passage refers to Marius, and one is here reminded of the passage already quoted referring to Verres (*omnibus in angiportis*). At first glance, then, *vici* would seem to be equivalent to *angiporta*, but it may signify "precinct" here. We know that it was in every precinct, not necessarily every street, that Augustus set up shrines to his *Genius* (cf. Shuckburgh on Suetonius *Aug.* 30).

"Street," however, is certainly the meaning of *vicus* in Horace *Epist.* i. 20. 17-18:

. . . te . . .

occupet extremis in vicis balba senectus.

Horace, incidentally, is one of the few authors who uses all three words, *angiportum*, *platea*, and *vicus*, meaning "street" or "precinct" (cf. Plautus and Vitruvius). Livy, who does not use *angiportum*, frequently cites *vici* with proper names (cf. Platner-Ashby, s.v. *vicus*), and explains the irregularity of *vici* at Rome in the following passage (v. 55. 4-5; cf. Diodorus xiv. 116):

Festinatiorum curam exemit vicos dirigendi, dum omisso sui alienique discrimine in vacuo aedificant. Ea est causa ut veteres cloacae, primo per publicum ductae, nunc privata passim subeant tecta, formaque urbis sit occupatae magis quam divisae similis.

*Vici* is here used clearly as a general term for streets, and the phrase *vicos dirigendi* is parallel to various phrases in Vitruvius (*platearumque et angiportuum . . . directiones* [i. 6. 1]; *directiones vicorum* [ibid. 6. 8]; *directionibus vicorum et platearum* [ibid. 6. 12]). Two passages in Tacitus are also interesting in this connection. They concern streets in Rome and the improvement effected after the fire of Nero: ". . . obnoxia urbe artis itineribus hucque et illuc flexis atque enormibus vicis, qualis vetus Roma fuit" (*Annal.* xv. 38); ". . . dimensis vicorum ordinibus et latis viarum spatiis . . ." (ibid. 43). Nipperdey takes *vici* in both passages to mean *Häuserreihen*; but we have seen above that this interpretation, derived from Varro, is suspect, although it must be admitted that the first of these two passages will bear this interpretation. But the meaning, "block of houses surrounded by streets," fits the context equally well if not better. In the second

passage the interpretation of *vici* as *Häuserreihen* is certainly strained. Here Furneaux translates "with rows of streets regularly measured out," comparing *Annales* i. 61. 3. In the second passage, if Furneaux's interpretation is correct, *vici* are contrasted with *viae*, as we might contrast streets and boulevards (cf. p. 58).

Noteworthy are the passages where *vicus* is modified by *angustus*, thus apparently taking the place of *angiportum* in its etymological meaning. This usage occurs in Petronius (*in vico angusto* [lx. 6]) and in Juvenal where, of course, the word *angiportum* could not be used for metrical reasons (6. 78):

.... per angustos figamus pulpita vicos ....

The narrowness of *vici* is stressed elsewhere in Juvenal (3. 236-37):

.... raedarum transitus arto  
vicorum inflexu ....

One may compare Martial (vii. 61. 3-4):

Iussisti tenuis, Germanice, crescere vicos  
Et modo quae fuerat semita, facta via est.

Here *vici* is used as a broad inclusive term, as it appears to be used in Vitruvius (cf. p. 55), while *viae* are contrasted with *semitae*. The same contrast is found in Cicero (*De leg. agr.* ii. 96; see n. 3). Neither *via* nor *semita* is exclusively applied to thoroughfares within cities as are *vicus*, *platea*, and *angiportum*, and this contrast between *via* and *semita*—usually road and path—is proverbial (cf. A. Otto, *Die Sprichwörter . . . der Römer*, s.v. *via*, Nos. 3, 4, and 5). Suetonius also stresses the narrowness of *vici*, even though he does not exclude *angiportum* from his writings: ". . . catervarios oppidanos, inter angustias vicorum pugnantis . . ." (*Aug.* 45); ". . . quasi offensus deformitate veterum aedificiorum et angustiiis flexurisque vicorum . . ." (*Ner.* 38). One may compare Tacitus, *Hist.* iii. 82: ". . . in partem sinistram urbis ad Sallustianos hortos, per angusta et lubrica viarum flexerant." Here *viae* is used instead of *vici* as a general term for streets.

This use of *vicus angustus* instead of *angiportum* is parallel to the use of *ρύμη στενή* instead of *στενωπός* in the *Koinē* (cf. n. 4 and text p. 57).

IV

Various passages are found where two of the three words now under consideration are used together. The most interesting series of such passages is found in Vitruvius:

... sequuntur intra murum arearum divisiones platearumque et angiporum ad caeli regionem directiones. Dirigentur haec autem recte, si exclusi erunt ex angiportis venti prudenter ... sed in angiportis et plateis non possunt consistere. ... [i. 6. 1].

Tum per angulos inter duas ventorum regiones et platearum et angiporum videntur deberi dirigi descriptiones. His enim rationibus et ea divisione exclusa erit ex habitationibus et vicis ventorum vis molesta. Cum enim plateae contra directos ventos erunt conformatae, ex aperto caeli spatio impetus ac flatus frequens conclusus in faucibus angiporum vehementioribus viribus pervagabitur. Quas ob res convertendae sunt ab regionibus ventorum directiones vicorum, uti advenientes ad angulos insularum frangantur repulsique dissipentur [*ibid.* 6. 7-8].

... quemadmodum ab impetu eorum aversis directionibus vicorum et platearum evitentur nocentes flatus [*ibid.* 6. 12].

... angiporum divisiones [*ibid.* 6. 13]. Divisis angiportis et plateis constitutis, arearum electio ad opportunitatem et usum communem civitatis est explicanda aedibus sacris foro reliquisque locis communibus [i. 7. 1].

Is autem locus est theatri curvaturae similis. Itaque in imo secundum portum forum est constitutum; per mediam autem altitudinis curvaturam praecinctionemque platea ampla latitudine facta, in qua media Mausoleum ... [ii. 8. 11, Halicarnassus].

... platearumque et vicorum uti emendate fiant distributiones in moenibus, docui et ita finitionem primo volumine constitui [iii. Praef. 4].

These passages include every occurrence in Vitruvius of each of the three words which are being considered in this paper. In these passages Morgan is consistent in his translations, using "alley" for *angiporum*; "street" for *platea*; and "row (line) of houses" for *vicus*. Choisy uses "rue" for *angiporum*; "quartier" for *vicus*; and "place publique" ("place"), "esplanade" (ii. 8. 11) or "avenue" (iii. Praef. 4) for *platea*. Granger varies his terms even more, using "alley" for *angiporum*; "street" or "broad street" or "main street" or "quarter of the city" (i. 6. 8) for *platea*; and "street" or "side street" (iii. Praef. 4) or "quarter" (i. 6. 12) for *vicus*. None of these translators, in my opinion, is correct in every instance. Vitruvius is anxious to

have the winds excluded from the *angiporta*—doubtless the narrow but numerous streets upon which the majority of the inhabitants would dwell, as we may infer from *ex habitationibus et vicis* (*ibid.* 8), which seems to correspond to *ex angiportis* (*ibid.* 1). *Plateae et angiporta* clearly include all streets and might best, perhaps, be translated “boulevards and streets” (thus preserving the foreign flavor of *plateae*) or “main streets and side streets.” But “streets and alleys” is not a satisfactory translation in modern English. *Platea*, of course, developed into “piazza” and “place,” etc., but I do not think that Choisy is here justified in translating it “place publique,” since in i. 7. 1, where *angiporta* and *plateae* are bracketed in the usual way, *areae* clearly designates the “places publiques”—a normal and common usage in this period.<sup>19</sup> Again, I can find no justification whatever for Granger’s translating *plateae* (i. 6. 8) “quarters of the city” though he has used “streets” only a few lines before. Nor has Morgan’s translation of *plateae et vici* (iii. Praef. 4), “streets and rows of houses,” anything except consistency to recommend it, for this phrase appears to be equivalent to *plateae et angiporta*. Furthermore, *vici* in the last sentence of the second quotation (i. 6. 8) seems to take the place of *plateae* in the previous sentence, and earlier in the same passage *ex habitationibus et vicis* seems to be merely an expansion of *ex angiportis* in the first quotation (*ibid.* 6. 1). It appears, therefore, that *vicus* is here used as a general term for street, and may be employed in place of either *platea* or *angiportum*.<sup>20</sup> It should be noted that the main thoroughfares (running from the walls or gates of the city to the forum, according to the conventional interpretation) are usually termed *plateae*; the cross-streets, *angiporta*.

*Angiportum* is bracketed not only with *platea*, as in Vitruvius, but also with *via*, as in Cicero (*De div.* i. 69): “. . . effusumque frumentum vias omnis angiportusque constraverat. . . .” A similar use is found in the *Bellum Alexandrinum* (ii. 4). On the other hand, Suetonius (*Jul.* 39) and Trajan (Pliny *Epist.* x. 32. 2) bracket or balance

<sup>19</sup> Cf. Horace *Carm.* i. 9. 18 (cf. Varro *LL* v. 38); *Forma urbis Romae*, 3 (cf. n. 15). *Area* is used in Plautus only as “fowling ground.”

<sup>20</sup> This loose use of terms in describing the thoroughfares of a city finds a good literary precedent in the description of Babylon by Herodotus (i. 180).



*vici* with *viae* (cf. Tacitus *Annal.* xv. 43, quoted p. 53).<sup>21</sup> More frequently *vici* is bracketed with *plateae* (Vitruvius i. 6. 12; iii. *Praef.* 4; Vegetius *Epit. rei mil.* iv. 25; Vulgate *Can. Can.* iii. 2; *Ev. s. Luc.* xiv. 21), and in one passage both terms seem to be included in the following *viae* (Caesar *Bel. civ.* i. 27. 3): ". . . portas obstruit, vicos plateasque inaedificat, fossas transversas viis praeducit. . . ." In this last passage *viae* seems to be a very broad generic term, but in the previous passages, where *viae* is bracketed with *angiporta* or *vici*, it would seem to have taken the place of *platea*—a word which appears to have been avoided by certain authors.

This use of *viae* to denote wide or important streets is amply attested by the passages from Cicero and Martial already cited, where there is a contrast between *viae* and *semitae*, and by the names of important thoroughfares in Rome, such as *Sacra Via*, *Via Nova* (there were two streets with this name), *Via Lata*, etc. This last street, *Via Lata*, is interesting since it takes the place of *platea* in its original meaning (cf. Herondas vi. 53), just as *vicus angustus* takes the place of *angiportum*. No doubt *platea* and *angiportum* had lost their etymological meanings in part, as actual usage indicates, and the phrases of noun plus adjective were more expressive. In the phrases *viae et vici* and *plateae et vici*, *vici* are doubtless the less important streets, like *angiporta* in similar phrases. *Vici* and *angiporta* seem to be equivalent also in the Catalogue of Constantinople (*Not. Const.*):

REG. I: vicos sive angiportus viginti novem.

REG. II: vicos sive angiportus triginta quattuor.

(The oldest manuscript, V, reads *angiportos* in both passages.) In the other regions *vici* is used alone. This indicates that in these two passages the phrase *sive angiportus* is added clearly to designate the *vici* as streets, not precincts, and that no distinction is being attempted between wide and narrow streets. The phrase includes all streets.

There is ample evidence, then, that *angiportum* is synonymous with *vicus* meaning "street" (cf. Vollmer, *Thes. L.L.*, s.v. *angiportum*) and

<sup>21</sup> In the passage from Pliny the phrase might very naturally be interpreted as meaning "(country) roads and (city) streets." As a rule in English and in Latin, terms applied to roads (*via*, cf. *semita*; Eng. "road," "lane," etc.) are occasionally applied to city streets, but terms for streets are not applied to country roads.

that it is just as incorrect to translate *angiportum* "alley" as it would be so to translate *vicus*. They should both be translated "street," with the understanding that the vast number of streets in Athens and in Rome were very narrow. *Platea* or *via*, when bracketed with *angiportum* or *vicus*, should be termed "avenue" or "boulevard" in order to distinguish between broad avenues and normal streets. Elsewhere *platea* and *via* and *vicus*, when used alone, may be generic terms for "streets." Occasionally *angiportum* is used in the same inclusive fashion.<sup>22</sup> The difference is not one of period (except that *vicus* meaning "street" may be somewhat later than these other words—the early evidence is ambiguous), but rather one of author. While it is true that *vicus* does seem to have replaced *angiportum* in most authors of the empire, *angiportum* is found in the fifth century. Various other words meaning "street" came in, as the Romance languages indicate, such as *strata* (Eng. "street"; cf. *CIL*, x, 4650), *ruga* (Fr. *rue*), etc.

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<sup>22</sup> This usage of any of these three words as a generic term for "streets" is paralleled by the usage of the corresponding Greek words (cf. n. 4).

## NOTES AND DISCUSSIONS

### ABOUT WINGED WORDS

Professor Calhoun's study of the Homeric formula *ἔπεα πτερόεντα* shows, as has so much of his late work, that he is one of the very few scholars with a real understanding of the nature of Homer's style.<sup>1</sup> Nevertheless—and it is the first time, I believe, that we have not rather generally agreed—he has failed to make me believe that I was mistaken in holding that Homer uses this phrase just because it is useful, and without thought for any particular meaning which the epithet “winged” might have. I stated my reasons for thinking this in an earlier number of this journal, but only in a single sentence, and I should now like to give my view more fully.

The various *ἔπεα πτερόεντα* verses, I believe, are used to bring in speech when “the character who is to speak has been the subject of the last verses, so that the use of his name in the line would be clumsy.”<sup>2</sup> Thus Homer could not have used at α 122 such a verse as τὸν δ' αὖ Τηλέμαχος πεπνυμένος ἀντίον ἦ᾽δ' α, as can be seen by reading from verse 113. The name of Telemachus is given in this verse, and it serves as the grammatical subject of all the following sentences in such a way that the second use of the name at 122 would break the style badly; what Homer wants to say is essentially “and he said,” not “and Telemachus said.” Likewise it cannot be only Homer's wish to get *πτερόεντα* in at ε 172 which keeps him from using some such verse as τὸν δ' ἄρ' ὑπόδρα ἰδὼν προσέφη πολύμητις Ὀδυσσεύς. He has just given the name of Odysseus in the verse before, and could not do so again. It is the same in all the other 124 *ἔπεα πτερόεντα* verses in the *Iliad* and the *Odyssey*: The hearer already has the speaker in mind as the natural subject of the sentence which introduces the speech, and there is no place for the second use of the name. Only in some five or six cases, where subordinate clauses with another subject have come between, might we again use the name without spoiling the style. Of course, if Homer had some other whole verse or verses without *πτερόεντα* in them whereby he could say “and he said,” there would be no purpose in pointing this out; but there is no other verse. If he wishes to express this idea in just the length of a verse, he is bound to use the words *ἔπεα πτερόεντα*. On the other hand, the phrase is never found in the same verse with the name of a character.

<sup>1</sup> “The Art of Formula in Homer—*ἔπεα πτερόεντα*,” *Classical Philology*, XXX (1935), 215–27.

<sup>2</sup> M. Parry, “The Traditional Metaphor in Homer,” *ibid.*, XXVIII (1933), pp. 39 f.

Thus, in order to hold that Homer uses the phrase only when he has in mind speeches of some given sort, one has to argue both that Homer never wanted to say in just a verse "and he said" and also that, when he wants to use *πτερόντα*, he plans the syntax ahead in such a way as not to have to give the name of the speaker when he introduces the speech. This would be a very complex sort of verse-making and quite foreign to the way in which such traditional and oral song as that of Homer is composed. The singer of oral narrative rarely plans his sentence ahead, but adds verse to verse and verse part to verse part until he feels that his sentence is full and finished. The poet, with writing materials, can think leisurely ahead, but the singer, in the speed of his song, must compose straight on out of fixed verses and verse parts until he comes to the point where one of his characters is to speak. Then he must have straightway at hand a verse or verse part to introduce the speech. This the common oral style has given him, as it has given him his diction as a whole; and it has given him not one or two formulas which he must in some way work in, but a whole living system of them which allows him each time to express just the right idea in a phrase of just the right words and length and rhythm. He has formulas to bring in a first speech in dialogue, or an answer, or to bring in monologue. He has formulas where there is place for the speaker's name, and others where the name, already understood, is implied in the verb. He has also formulas which simply bring in the speech, or which also state the tone, or which give the name of the person spoken to, or give some circumstance about the speaking. Finally, within these categories of meaning he has formulas of different grammatical form to fit the grammatical sequence of the passage, and of different metrical forms to fit into his verse at the different points where he may find himself. However, he usually has only one formula to suit a particular need, since the earlier singers in their natural and never ceasing search for the easiest means of verse-making had usually kept and passed on to him only as many fixed phrases as were really useful. Thus Homer, when he has just a verse to fill and wishes to express the idea "and he said," will use the simple formula *καί μιν (σφείας) φωνήσας (φωνήσας)* *ἔπεα πτερόντα προσήυδα (προσηύδων)* 54 times. Or, if he wishes also to give the tone of the speech, he will use a formula of the type:

[11 times] *καί ρ' ὀλοφύρόμενος* } *ἔπεα πτερόντα προσήυδα*  
 [4 times] *καί μιν λισσόμενος* }

Or he may state some circumstance:

[13 times] *ἀγχοῦ δ' ἰστάμενος* } *ἔπεα πτερόντα προσήυδα*  
 [3 times] *τοὺς δ' γ' ἐποτρύνων* }

Finally, there are times when he finds himself ready to announce speech though he is only halfway through a verse. He then says, simply, *ἔπος τ' ἔφατ' ἔκ τ' ὀνόμαζε* (45 times). It is for purely grammatical reasons that we have *ἔπος τ' ἔφατ' ἔκ τ' ὀνόμαζε* and not *ἔπεα πτερόντα προσήυδα* in such a

verse as  $\epsilon\upsilon\tau'$  ἄρα οἱ φῦ χειρὶ ἔπος τ' ἔφατ' ἔκ τ' ὀνόμαζε (11 times). Now to find in  $\pi\tau\epsilon\rho\acute{o}\nu\epsilon\tau\alpha$  and  $\delta\acute{o}\nu\alpha\zeta\epsilon$  some meaning which would limit the use of these formulas to speeches of some one sort is to take away a whole part from the system and say that Homer has no speech formulas for the verse and the half-verse—the most common measures of the formula—which simply mean “and he said.”<sup>3</sup>

When Professor Calhoun gives the particular meaning which he finds in  $\epsilon\pi\epsilon\alpha$   $\pi\tau\epsilon\rho\acute{o}\nu\epsilon\tau\alpha$  and  $\epsilon\pi\omicron\varsigma$  τ' ἔφατ' ἔκ τ' ὀνόμαζε, he says that the range of the two expressions overlap, and that they express “myriad facets of the human spirit,” and finally that “the one usually connotes animation or urgency, the other earnestness or affection.”<sup>4</sup> Is it not possible, however, that he has here for once fallen into somewhat the same line of reasoning as that so often followed by the so-called “Unitarians?” This school of critics thought that the only way to defend Homer against the disintegrators' charge of inconsistencies in the text was to show that there were really no inconsistencies at all. They accordingly gave their ingenuity full play and looked for subtle beauties of thought which had escaped the dull understanding of Homer's belittlers, who through their lack of poetic feeling had seen inconsistencies where there was only poetic refinement. Professor Calhoun, of course, is far beyond this; yet is it not, perhaps, a like unwillingness to believe that Homer might have used common formulas, and used them without thinking about what the words in them meant, which has led him to find in all the speeches introduced by  $\epsilon\pi\epsilon\alpha$   $\pi\tau\epsilon\rho\acute{o}\nu\epsilon\tau\alpha$   $\pi\rho\omicron\sigma\eta\acute{\iota}\delta\alpha$  an emotion and intensity which would set them off from the other speeches of the poems? Certainly when he paints for us in his own words the circumstances and the substance of each one of these speeches he sets vividly before us its intensity and emotion. But here I must fall back on the somewhat questionable charge of  $\delta\epsilon\iota\nu\acute{o}\varsigma$  λέγειν, because I believe that the fallacy of the method lies in the fact that another critic, if he knew how to write as well as Professor Calhoun, could paraphrase in the same lively way the speeches introduced by any other group of formulas. Is there not everywhere in the *Iliad* and the *Odyssey* enough of the simple force of Homer's heroic style to allow a writer of talent to bring forcibly before us the intensity and emotion of any given passage? Professor Calhoun thus places his readers in the plight of having to agree with him before they can disagree, save perchance in the few cases where one can argue against the sort of intensity and emotions which he finds. Thus he tells us that Eteoneus (δ 20 ff.) has his hands so full with the banquet that the sight of Telemachus and Pisistratus, two new and uninvited guests,

<sup>3</sup> No small part of my knowledge of  $\epsilon\pi\epsilon\alpha$   $\pi\tau\epsilon\rho\acute{o}\nu\epsilon\tau\alpha$  I owe to my former students, J. P. Cooke and M. V. Anastos, who made a study of the length of the speeches introduced by the different speech formulas. This they did to disprove an explanation of  $\epsilon\pi\epsilon\alpha$   $\pi\tau\epsilon\rho\acute{o}\nu\epsilon\tau\alpha$  more improbable even than Mr. Calhoun's—namely, that the phrase is used to introduce short speeches.

<sup>4</sup> Pp. 225 f.

is for him "the last straw," and he "momentarily loses his head" and announces their arrival in winged words, but I do not believe that every reader of the *Odyssey* will be willing to find this comedy between the lines for it. Likewise I myself do not like to think of Zeus as being "eager and brisk" when he gives orders to the baneful dream (B 7). Such a Zeus seems to me too little Phidian.

Professor Calhoun uses as one of his arguments the fact that "out of somewhat more than 120 instances of *ἔπεα πτερόεντα* more than 70 are preceded by explicit allusions to emotion or its symptoms,"<sup>5</sup> but is it not also true that the speech formulas of the type ending in *προσέφη πόδας ὠκὺς Ἀχιλλεύς, προσέφη πολύμητις Ὀδυσσεύς*, and so on (if one puts aside verses of the type *τὸν δ' ἀπαμβίβωμενος προσέφη πόδας ὠκὺς Ἀχιλλεύς*, which is limited to answers) show an equally high number of cases where there is some word which we could class as emotional? What difference is there between *καί μιν ὑπόδρα ἰδὼν ἔπεα πτερόεντα προσηύδα* (twice) and *τὸν δ' ἄρ' ὑπόδρα ἰδὼν προσέφη πολύμητις Ὀδυσσεύς* (9 times) save the presence of the name in the one case and its absence in the other, and the difference for grammatical reasons of the conjunction? We can, perhaps, see here what has led Mr. Calhoun to his conclusions: having found in the text, either in the same verse as *ἔπεα πτερόεντα* or very near it, a large number of such words as *νεμεσσήθη, ῥίγησεν, γήθησεν*, and so on, has he not supposed that the meaning of these words must accord with the meaning of the epithet *πτερόεντα*? By the same reasoning, however, we could argue equally well for an emotional connotation of *ἔπεα* and of *προσηύδα*. One can be somewhat surer that he has thus reasoned his way to his understanding of *ὀνόμαζεν* as connoting "earnestness or affection." This half-verse is most often found in the following whole verse formulas:

[6 times]	<i>χειρὶ τὲ μιν κατέρειξεν</i>	} <i>ἔπος τ' ἔφατ' ἔκ τ' ὀνόμαζε</i>
[11 times]	<i>ἔν τ' ἄρα οἱ φῶ χειρὶ</i>	

and in these verses there are doubtless earnestness and affection. However, we also have

[4 times]	<i>Ἀντίνοος</i>	} <i>δ' ἐνέειπεν ἔπος τ' ἔφατ' ἔκ τ' ὀνόμαζεν</i>
[π 417]	<i>Ἀντίνοον</i>	
[τ 90]	<i>ἄμφίπολον</i>	
[ψ 96]	<i>Τηλέμαχος</i>	
[O 552]	<i>τόν ρ' ἔκτωρ ἐνέειπεν ἔπος τ' ἔφατ' ἔκ τ' ὀνόμαζε.</i>	

Is there not, therefore, as much reason to say that *ὀνόμαζε* connotes dislike as to say that it connotes affection? Have we the right to suppose, in the one case more than in the other, that the meaning of the first half of the verse has anything to do with the meaning of the second half?

These are the particular reasons I have for thinking that Homer used *πτερόεντα* and *ὀνόμαζεν* without thinking of their special meaning; but the

<sup>5</sup> P. 225, n. 1.

issue at stake here is one which probably stands beyond such minute arguing. It seems to me to be the whole issue of whether we should read Homer as we read written poetry, which is for us the natural form of poetry, or whether we should not rather try to gain for our reading the sense of style which is proper to oral song. I know from my own mistakes that this is no easy thing. In some measure we may gain this sense by our mere feeling for the diction of the Homeric poems, but there is still then the danger of looking too closely and finding beauties where they are not. The reading of the *Iliad* and the *Odyssey* must be abetted by much reading of the other early European heroic poetries, and by the study of some of the many oral narrative poetries which still thrive in those places of the world where reading and writing have as yet gained no hold. The Homeric student who does this will come back to the *Iliad* and the *Odyssey* better able to feel their conventional wording as the usual heroic language of a tale which ever sweeps ahead with force and fineness, but also with an obviousness which is so utter that it may deceive, as I believe has been the case for Professor Calhoun, even the best of critics.

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#### THE VERSE STRUCTURE OF *EPIDICUS* 25-26

W. M. Lindsay, in his edition of the comedies of Plautus, prints *Epidicus* 25-26 as follows:

TH. ius	25
dicis. EP. me decet. TH. iam tu autem	25a
nobis praeturam geris? EP. quem	25b
dices digniorem esse hominem hodie Athenis alterum?	26

and scans 25a-26 as trochaic.<sup>1</sup> All other recent editors print the passage in two lines, and scan the two verses as iambic octonarii; to facilitate this scansion they place *quem* at the beginning of 26, although A and the Palatine manuscripts agree in making *quem* the final word of the preceding line.

The reading and scansion of 26 have been particularly troublesome. The verse appears in A as

DICTISDIGNIOREMESSEHODIE . . HEN . . ALTERUM,

but the Palatine MSS agree upon the reading:

Dices digniorem esse hominem hodie Athenis alterum,

a verse which can be scanned only with hiatus after *hominem*.<sup>2</sup> Gray and

† Deceased.

<sup>1</sup> Cf. also Lindsay, *Early Latin Verse* (Oxford, 1922), pp. 330, 352.

<sup>2</sup> The refusal to accept this hiatus has led to numerous emendations of the verse: C. Fuhrmann ("Die Vergleichungssätze bei Plautus," *Jahrb. f. d. Phil.*, XCVII [1868], 851) reads *digniorem (me) esse*, scanning as an iambic octonarius, but suggests *hodie* for *hodie* as a less probable alternative; cf. also R. Müller (*De Plauti Epidico* [Bonn, 1865], p. 31): *quem hominem dices digniorem esse hodie*; C. F. W. Müller (*Plautinische Prooedie* [Berlin, 1869], p. 617): *esse (aeque) hominem*; T. Hasper (*Ad Epidicum*



Goetz read *hominem* <hoc> *hodie*;<sup>3</sup> all other editors from Ussing on accept the hiatus and follow the reading of the Palatine manuscripts.

The first scene of the *Epidicus* (1-103) is a *canticum* composed for the most part of iambs and trochaics, with a number of cretics in the latter part of the scene (cf. especially 85 ff.). Many verses are difficult to scan, and there has been a constant attempt on the part of scholars to explain the meter of individual verses and to clarify the structure of the *canticum* as a whole.<sup>4</sup> In 1929 Crusius<sup>5</sup> contributed materially to an understanding of two verses (9 and 29) when he pointed out the strophic arrangement of 7-11 and 27-31. His analysis of these two passages is as follows:

Zwei jambische Oktonare (oder vier Dimeter, wegen des Hiats in der Dihärese des Verses 27), zwei kretische Monometer, sechs jambische Metra, zwei Lekythia. Die Uebereinstimmung von Strophe und Antistrophe wird äusserlich bestätigt durch quid ceterum 7 und quidnam 27 an fast entsprechender Stelle und die entsprechende Frageform der kretischen Monometer. Auch inhaltlich besteht Uebereinstimmung: In Vers 7 f., wie in Vers 27 f. wird Epidicus von Thespro an der Nase geführt.<sup>6</sup>

The important fact for consideration here is Crusius' scansion of 9-10 and 29-30: two cretics followed by a sequence of twelve iambic feet.<sup>7</sup> This explanation solves the difficulties which had made both 9 and 29 a source of trouble to students of Plautine meter.<sup>8</sup> The presence of cretics in 29 had formerly been suspected,<sup>9</sup> but Crusius was the first to point out the cretics in 9 and to show the similarity of the two passages.

*Plautinam coniectanea* [Dresden, 1882], p. 10): *diceis esse digniorem (me) hodie*, or (retaining *hominem*): *esse hominem (me) hodie*; Goetz-Schoell (*Praef.*, p. xi): *quem digniorem dices esse*.

<sup>3</sup> Cf. L. Havet, "Observations sur Plaute," *Rev. phil.*, XXXII (1908), 8.

<sup>4</sup> Cf. R. Müller, *op. cit.*, pp. 24-41; A. Spengel, *Reformvorschlge zur Metrik der lyrischen Versarten bei Plautus* (Berlin, 1882), pp. 369 f.; F. Leo, "Ein Kapitel Plautinischer Metrik," *Rhein. Mus.*, XL (1885), 181 ff.; H. Roppenecker, *De emendatione metrica canticorum Plautinorum* (Freising, 1894), p. 33; Leo, *Die Plautinischen Cantica und die hellenistische Lyrik* (Berlin, 1897), pp. 9 ff., 30, 99 (= *Abhand. k. Gesell. Gtt.*, I, 7); V. C. Lindstrm, *Commentarii Plautini* (Uppsala, 1907), pp. 86 ff.; Havet, *op. cit.*, p. 11; S. Sudhaus, *Der Aufbau der Plautinischen Cantica* (Leipzig, 1909), p. 66; Roppenecker, "Vom Bau der Plautinischen Cantica," *Philol.*, LXXXV (1930), 69 ff.

<sup>5</sup> F. Crusius, *Die Responsion in den Plautinischen Cantica* (Leipzig, 1929) (= *Philol.*, Suppl. XXI, Heft 1).

<sup>6</sup> *Ibid.*, pp. 102 f. The responsion in 7-11 and 27-31 is accepted by A. Klotz (*Phil. Woch.*, L [1930], 907) and J. Safarewicz (*Rev. phil.*, LVI [1930], 376, 380).

<sup>7</sup> On iambic "runs" cf. Lindsay, *Early Latin Verse*, p. 278.

<sup>8</sup> Both 9 and 29 have been scanned by some editors as iambic, by others as trochaic. Goetz, e.g., emends 29 to read: *quid (est quod) rogas?* Goetz-Schoell and Ammendola list 29 among the verses that are *incerti*.

<sup>9</sup> Cf. Leo, *Die Plautinischen Cantica*, pp. 9 f. (although he had earlier scanned 29 as iambic; cf. *Rhein. Mus.*, XL [1885], 181 ff.); E. Redslob, *Berl. Phil. Woch.*, XXII (1902), 553; Lindstrm, *op. cit.*, pp. 86 f.

The Ambrosian palimpsest gives a curious colometry for 9:

QUID  
TUAGISUTUALES  
EXEMPLUMADESSEINTELLEGO EUGAE

This indicates, at least, that 9 in A was written in three lines, and may show that originally the words *quid tu agis? ut uales?* were written as cretics and separated from the remainder of the verse. The corresponding verse (29) is unfortunately missing in A, and we shall probably never know whether it was originally written in three lines. But, strangely enough, A has for 25 an arrangement similar to that for 9:

IUS  
DICIS MEDECET  
IAMTUAUTEMNOBISPRAETURAMGERIS QUEM

Does this mean that the scansion of 25 is similar to that of 9? Crusius thought not, for he accepted the traditional scansion of 25-26 as iambic octonarii.<sup>10</sup>

Let us, however, examine 25-26 more closely. The two short speeches at the beginning of 25 (*ius dicis, me decet*) can well be cretics.<sup>11</sup> Thesprio's next speech forms five iambic feet. The reply of Epidicus (*quem dices digniorem esse hominem hodie Athenis alterum?*) is, as I have said, usually scanned as an iambic octonarius. But this scansion is based upon the reading of the Palatine manuscripts. The speech in A reads: *quem dices digniorem esse hodie Athenis alterum?*—a series of seven iambic feet. In other words, if we accept the reading of the palimpsest and omit the superfluous *hominem* of P, the two verses may be scanned as two cretics followed by a sequence of twelve iambic feet, and the likeness of 25-26 to 9-10 and 29-30 becomes at once apparent.

The scansion of 25-26 as a system of cretics and iambs helps, therefore, to explain the four-line arrangement of A and makes it possible for us to accept the reading of A in 26. Moreover, the words *ius dicis, me decet* of 25 in the Palatine manuscripts are written at the end of 24, a short verse; if the two cretics were written in the archetype in separate lines (as 85, 87, 89, etc. in A), it is not surprising that a scribe, in order to save space, should have placed them at the end of the preceding verse.<sup>12</sup> For these reasons, I believe, the two verses should be printed as follows:

TH. *ius dicis.*  
EP. *me decet.*  
TH. *iam tu autem nobis praeturam geris? EP. quem*  
*dices digniorem esse hodie Athenis alterum?*

<sup>10</sup> *Op. cit.*, p. 103.

<sup>11</sup> The molossus (*ius dicis*) is an acceptable substitute for the cretic, especially in the first foot (cf. Lindsay, *Early Latin Verse*, pp. 291 ff.).

<sup>12</sup> Both 8 and 28 are written in the MSS as long verses; the two cretics in 9 and 29 could not, therefore, be placed at the end of the preceding verses.



Lindsay, the transposition *arma perdidit*, the verse need no longer create metrical difficulties, or give rise to a large number of additional emendations, as it has in the past.<sup>19</sup> The two cretics at the beginning of 57 (*Epidice perdidit*) are followed by four iambic feet (*me. quis? ille qui arma perdidit*); if we add to these the eight iambic feet of 58, we have a sequence of two cretics followed by twelve iambs—the same system which has already been shown to exist in 9–10, 25–26, and 29–30. The passage should perhaps be printed as follows, to bring it into conformity with the corresponding passages earlier in the scene:

Epidice?

Er. perdidit

me. Th. quis? Er. ille qui arma perdidit.

Th. nam quid ita? Er. quia cottidie ipse ad me ab legione epistulas.

Furthermore, the possibility that 57–58 contain the same metrical scheme that Plautus has previously used gives added weight to the soundness of the reading *arma perdidit*.<sup>20</sup>

Plautus' fondness for cretics in the latter part of the first scene of the *Epidicus* has long been recognized. Crusius, by paving the way to a clearer understanding of the systems of cretics and iambs in the first part of the *canticum*, has done a signal service for Plautine scholars. Plautus' use of this system must now be extended more widely;<sup>21</sup> 25–26, and probably 57–58, are further illustrations of his tendency to use a combination of two cretics and a "run" of twelve iambic feet.

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## TWO NOTES ON EURIPIDES

### A. SKEPTIC OR PATRIOT?

A small dilemma confronts the reader of the *Hecabe*. In the exposition of the play we are told that the ghost of Achilles has appeared to all the Greeks<sup>1</sup> to demand the sacrifice of Polyxena;<sup>2</sup> that a debate has ensued in the Greek assembly about this demand, in which Agamemnon argues against the sacrifice, and the sons of Theseus for it, until Odysseus swings the assembly

<sup>19</sup> Cf. n. 13.

<sup>20</sup> It must be mentioned, however, that A gives all of 57 in one line; there is no attempt to separate the cretics from the remainder of the verse, as apparently in 9 and 25.

<sup>21</sup> The desire to find cretics in this *canticum* should not be carried to extremes; Reddlob (*op. cit.*, col. 553), Lindström (*op. cit.*, pp. 138 f.), and Havet (*op. cit.*, p. 10) scan 31 as cretics (*serion dicis tu*) followed by trochaics, in violation of the *serione* of the MSS. The comparison of 27–31 with 7–11 is helpful here. Both 11 and 31 are composed of trochaic dimeters catalectic.

<sup>1</sup> Ll. 37–41, 92–95, 109–15.

<sup>2</sup> L. 40.

to the latter view.<sup>3</sup> The sons of Theseus create the dilemma. Does Euripides regard human sacrifice as justifiable? Surely a man so humane and so un-superstitious cannot. But then, does he go out of his way to put the Athenian chiefs in the wrong? Gilbert Murray<sup>4</sup> feels that he does; the introduction of the Theseids is, then, a slap at Athens by a disillusioned patriot, rebelling against the faults of his beloved country. Though this interpretation carries weight because of its source, I cannot accept it.

In the above-cited expository passages the poet states objectively the facts of his play; no feeling appears but the inevitable recoil of Hecabe and the chorus. The following scene,<sup>5</sup> however, is the focus for the appearance of the poet's sentiments. Odysseus comes to lead Polyxena to her death. Hecabe pleads her cause to him on behalf of her daughter. First, she claims his gratitude—a claim he acknowledges is just; she then argues against human sacrifice, and, in particular, the choice of Polyxena. Odysseus is chiefly concerned to defend his advocacy of the sacrifice. He does so on the ground that gratitude to benefactors is a cardinal virtue for cities, and therefore Achilles must receive honor from the Greeks.

What impression should we derive from this scene? Our sympathy is with Hecabe, as Euripides intended; so much is clear. Is Odysseus, therefore, in the wrong? I think not. He cuts a poor figure, it is true; in two respects, perhaps, he resembles the slippery schemer of the *Philoctetes*, *Trojan Women*, and elsewhere—in his sophistical implication that he is bound to protect Hecabe, but not her daughter, and in his prudent preparations to avoid Polyxena's supplication.<sup>6</sup> But this is the worst that can be said of him, and perhaps worse than need be said. His point of policy is entirely sound and admirable. Surely, then, we cannot expect him to put a private obligation ahead of the public interest. Agamemnon does just this, thereby proving that Euripides takes the king as he finds him in the *Iliad*. But could the poet possibly approve such conduct? I think not; to subordinate one's self, even one's virtues, to the state was recognized as a virtue in Greece. Only an Aristides or a Socrates, perhaps, perfectly honored the principle; but only an Alcibiades would argue against it. The worst, then, of Odysseus' position is its awkwardness and dramatic thanklessness.<sup>7</sup>

The reality of the ghost of Achilles is unquestioned in the play. I see not the slightest hint that the Achaeans are deluded or "too superstitious."<sup>8</sup> In-

<sup>3</sup> Ll. 116-40.

<sup>4</sup> *Euripides and His Age*, pp. 89-90; and cf. p. 126: "It is against the gods and Athens that his irony cuts sharpest."

<sup>5</sup> Ll. 216-443.

<sup>6</sup> Ll. 301-2 and 342-45, respectively.

<sup>7</sup> Some significance may be attached to the order of speeches in this scene; see below, Part B.

<sup>8</sup> Murray, *op. cit.*, p. 89, thinks the army are victims of mob-hysteria but, naturally in so brief a discussion, does not cite evidence.

deed, it would be hard to cast doubt on ghosts in a play for which a ghost speaks the prologue.<sup>9</sup> Polydorus is an entirely honest ghost, and so his authentication of Achilles (ll. 37-41) is valid. We are required to believe that Achilles appeared and demanded Polyxena. Odysseus, therefore, is quite right in advising the sacrifice; and if Odysseus is right, the sons of Theseus are right, and their inclusion is a patriotic touch such as abound in Euripides' plays.

This result entails an interesting corollary. Euripides has accepted for purposes of this play a piece of machinery, the ghost, and a practice, human sacrifice, about both of which we would expect Euripides to be at least skeptical, if not definitely hostile. This is a small bit of evidence toward a conclusion—for which far more ample foundations are available—that Euripides varied from play to play and did not have a system of principles which he consistently preached. The patriotic touch appears as consistently as anything in the way of specific attitudes; the foundation here was probably genuine feeling, but the specific phenomena are chiefly good "box office," elsewhere as in the *Hecabe*. Such phenomena should, I think, always be taken at their face value; thus, for the *Ion* I can accept neither Verrall's view that Athena is bogus,<sup>10</sup> nor Murray's, that Euripides is attacking the legends of Athens as sham glories. Since Euripides takes the pains to make Dorus junior to Ion,<sup>11</sup> it seems that we must understand that he meant to flatter Athens in this play, and that here, as elsewhere in Euripides, love of country is a sentiment more dependable and less fluctuating than his view on ethics and theology.<sup>12</sup>

#### B. DEBATES

In Euripides' debates, the prevailing speech naturally is placed second, in climactic order; usually the prevailing speech is also the better—that is, the debate is won on merit. Exceptions to this rule are *Cyclops*, ll. 285-346 (Odysseus versus Cyclops, for comic effect), *Hippolytus*, ll. 372-481 (Phaedra versus the Nurse, who will not be convinced by the arguments approved as better by the chorus [ll. 482-85]), and *Troades*, ll. 634-705, where Hecabe seems for the merest moment to have argued better. In *Andromache*, ll. 590-746, Menelaus has the advantage of position over Peleus, which makes his craven collapse the more obvious. In numerous cases, while the second speaker is superior, only the first speaker could introduce the debate; such cases are *Alcestris*, ll. 629-705 (Admetus versus Pheres), *Heracleidae*, ll. 134-231 (Herald versus Iolaos), *Hippolytus*, ll. 936-1035 (Theseus versus Hippo-

<sup>9</sup> Polydorus' ghost appears in a dream to Hecabe (cf. ll. 54 and 68-78, and W. S. Messer, *The Dream in Homer and Greek Tragedy*, pp. 85 ff.) like the ghost of Patroclus (*Iliad* xxiii. 62-100).

<sup>10</sup> Verrall's edition of the *Ion*, pp. xvii-xix and xxxv.

<sup>11</sup> *Ion*, ll. 1589-90; cf. Apollodorus, i. 7. 3, and Harrison and Verrall, *Monuments and Mythology of Athens*, p. lxxxi.

<sup>12</sup> In the *Hecabe* note another patriotic allusion, ll. 466-74.

lytus), *Andromache*, ll. 155–231 (Hermione versus Andromache), *Hecabe*, ll. 1132–1237 (Polymestor versus Hecabe), *Suppliants*, ll. 409–62 (Herald versus Theseus), *Heracles*, ll. 140–235 (Lycus versus Amphitryon—the specific debate on the merits of archery is won by Amphitryon, but we may agree with Verrall<sup>13</sup> that he does not prove the whole case for Heracles), *Troades*, ll. 914–1032 (Helen versus Hecabe), *Iphigenia in Tauria*, ll. 674–710 (Pylades versus Orestes), and *Orestes*, ll. 491–604 (Tyndareus versus Orestes). The scene with Pentheus, Tiresias, and Cadmus in the *Bacchae* (ll. 215–369) does not center on a well-defined point, but the right side speaks second. In some of these plays we could imagine the second speaker introducing the debate if his position were not superior—if he were made by the poet to “protest too much” (e.g., *Hecabe*, *Troades*, *Orestes*). Also we see that when the right side has to introduce the debate, Euripides sometimes allows a definite rebuttal to indicate the weakness of the second speaker; this appears in *Medea*, ll. 465–587 (Medea versus Jason) and *Phoenissai*, ll. 469–585 (Polynices versus Eteocles; rebuttal by Jocasta). In three plays the order of speakers could easily be reversed if the second speaker were not on the right side; these are *Heracles*, ll. 1255–1339 (Heracles versus Theseus), *Electra*, ll. 1011–1099 (Clytemestra versus Electra—the same order appears in Sophocles’ *Electra*), and *Iphigenia at Aulis*, ll. 334–401 (Menelaus versus Agamemnon). Also in the reported debate of *Orestes*, ll. 884–952, the order of speakers is: Talthylbius, Diomedes, the Demagogue, the Honest Yeoman, Orestes. The defense is thus matched with the prosecution in the advantageous position. This analysis creates a strong presumption that in the *Hecabe* the second speaker, Odysseus, is recommended to us by the poet.

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#### THE ANTHOLOGY OF VALERIUS MAXIMUS AND A. GELLIUS

The epitomes and excerpts from Valerius Maximus which attest the popularity of that author during the Middle Ages bring added problems to the tradition of his text. Those arising from the anonymous Anthology of Valerius and Gellius, as preserved in Vaticanus Latinus 3307, saec. XII (y)<sup>1</sup> have already been handled by Valentini,<sup>2</sup> who concludes that the codex is derived directly from neither of our two early manuscripts of Valerius, but that it is connected very closely with them and represents perhaps a more correct revision of the text than is given by their first hands. Valentini’s manuscript,

<sup>13</sup> *Four Plays of Euripides*, pp. 146–51.

<sup>1</sup> I have adopted the small letter, rather than the Y of Valentini, in accordance with my previous practice of designating by small letters manuscripts containing excerpts from Valerius, and by capitals those containing the full text.

<sup>2</sup> *Classici e Neolatini*, VI (1910), 251–77.



however, is a palimpsest and not entirely satisfactory; it fails the editor often where the older script has been poorly erased and the newer script of the Anthology thus rendered illegible. In addition, portions of its folios have been eaten away by worms. It is well, then, to bolster Valentini's conclusions by reference to two additional manuscripts of the Anthology which are now accessible to us.

Of these the first is Parisinus Latinus 4952 (t), also of the twelfth century, known to both Valentini and Hertz,<sup>3</sup> but actually consulted only by the latter, who collated the passages of Gellius contained therein for his edition of that author. The Anthology is found in its complete form on folios 136v-203 of this manuscript, under the heading *Ex Maximi Valerii et Agellii libris excerpta*.

The second is apparently identical with the Codex Iacobaeus which Hertz,<sup>4</sup> and Valentini following him, assumed to be lost to modern scholars. The codex was listed in the *Catalogi librorum MSS. Angliae et Hiberniae in unum collecti*<sup>5</sup> among the Jacobean volumes under the title *Anonymi excerpta ex A. Gellio et Valerio Maximo. Cicero de officiis liber I*, and the number 8594.872. It is certain, likewise, that Richard Bentley made use of it, since in his copy of the *editio Stephaniana* of Gellius (1585), now preserved in the British Museum,<sup>6</sup> he noted at i. 26 the following: *Excerpta MS. codicis Regii sti Jacobi 700 annorum*; that this is the same codex which contained the Anthology is a reasonable conclusion since the chapter with which Bentley is concerned is included in our Anthology. Casley, however, cataloguing the manuscripts of the King's Library in 1734, found no such codex, and Dziatzko, searching at Hertz's request, did not discover it among the volumes of the British Museum where it might most naturally have gone.

This manuscript I identify with Cantabrigiensis Trin. Coll. R. 16. 34 (s), saec. XII, containing the extracts from Valerius and Gellius, a short poem from the *Anthologia Latina*, selections from Seneca's *De beneficiis*, and the first book of Cicero's *De officiis*.<sup>7</sup> Judging merely from the contents, the identification is logical, even though the second and third items are not listed in the *Catalogi librorum MSS. Angliae et Hiberniae*. But the identification becomes even more certain when we read in the modern catalogue of Trinity College Library<sup>8</sup> that this manuscript was donated to the college by Bentley himself in the year 1757.

The Anthology with which we are concerned is found on folios 1-22v; much of it is in better condition than the one of the Vatican manuscript, but

<sup>3</sup> Ed. Gellius, II (1885), li.

<sup>4</sup> *Ibid.*, p. liv.

<sup>5</sup> II (1697), 247.

<sup>6</sup> Cf. A. Stachelscheid, in *Rh. Mus.*, XXXV (1880), 633-34, and Hertz, *op. cit.*, p. liv.

<sup>7</sup> The second book, begun on fol. 43v, breaks off abruptly on fol. 44v with chap. ix, and there is no trace of the third book.

<sup>8</sup> Montague Rhodes James, *The Western MSS. in the Library of Trinity College, Cambridge*, II (1901) (MSS. in Class R), 396.

it is imperfect at beginning and end. The text starts on folio 1 in a story of Lais from Gellius i. 8, with the result that the introduction of the excerptor, the preface of Valerius, Gellius i. 3. 23, and Valerius i. 1. ext. 3-4 are missing; it finishes, not as do the other two manuscripts with a hundred passages drawn from Gellius without similar borrowings from Valerius, but with only six chapters from this author (xiii. 5, xvi. 1-2, xix. 2-4).

Instead of the one manuscript used by Valentini we have, therefore, three manuscripts<sup>9</sup> of about the same period containing the Anthology of Valerius and Gellius. That no one of these manuscripts has been copied from either of the other two can be shown by the following passages, in each of which one manuscript omits a reading which is transmitted complete by the other two: Kempf<sup>10</sup> p. 68, 2, *ideoque . . . est habitus* om. t; 85. 12, *instrueras . . . non tuo* om. s; 94.2, *quo nomine* om. s; 98.5, *nosse* om. t; 125.27, *pondere* om. y; 130.10, *in admirationem . . . tuos Muci* om. s; 196.5, *civitas* om. t; 214.18, *quid multa* om. y; 284.9, *superstes . . . debitam sibi* om. s; 386.17, *consummatio* om. t; 388.1, *inter totius* om. y. Yet all three are unanimous in supporting the readings cited by Valentini in proof of the importance of the Anthology to the Valerian tradition. His conclusions, with our new evidence, may be stated as follows: s, t, and y sometimes agree with L (Laurentianus 1899, saec. IX) against A (Bernensis 366, saec. IX), sometimes with A against L. They have, however, very little in common with the emendations of A<sup>2</sup> especially with those drawn from Paris, and in the few agreements which do occur, L<sup>1</sup> or L<sup>2</sup> will generally furnish the same reading. It follows, then, that the Anthology may be of value in preserving readings derived from the archetype of AL. That it should be considered of value also as offering emendations in its own right is suggested by the following reading: Kempf (224.8) has *quodam*, after the emendation of Freinsheim and Gertz, now confirmed by sty, whereas A<sup>1</sup>L<sup>1</sup> Halm have *quosdam* and A<sup>2</sup>L<sup>2</sup> *apud quosdam*. The Anthology, with other epitomes and excerpts in prose and verse, cannot be neglected by the future editor of Valerius.

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#### CONCERNING THE *SECOND SALLUSTIAN SUASORIA*

The present trend in Sallustian criticism seems to be running strongly in favor of regarding the *Second Suasoria* as being really by Sallust. During the

<sup>9</sup> It is necessary to mention finally a manuscript already cited by Hertz (*op. cit.*, p. liv) as containing fragments from the Anthology. Codex acad. Bonn. 218, a parchment miscellany written in hands of the eleventh century, included two blank pages which were afterward filled by a hand of the thirteenth century with excerpts from Valerius and Gellius. It is difficult, however, to determine what order the scribe was following when he made his selections; the text, moreover, is so stenographic and in many cases so garbled that a comparison with the tradition of sty is unsatisfactory.

<sup>10</sup> *Valerii Maximi factorum et dictorum memorabilium libri novem*<sup>2</sup> (Teubner, 1888).

last five years not a few works on Sallust have been published in Germany, and all agree that Sallust probably did write the *Second Suasoria*. This marks quite a change; less than fifteen years ago scholastic opinion was decidedly dubious about the authenticity of the pamphlet. One recalls the paper by Professor Hugh Last in *Classical Quarterly*, XVII (1923), 87-100, 151-62. One of the first students of the subject to attempt to refute Professor Last's views was Professor Post of Haverford (in *Classical Weekly*, XXI [1927], 19-23); and it must be a source of satisfaction to him that the Germans share his viewpoint. At the same time Professor Post may perhaps feel disappointed that in one direction the supporters of Sallustian authorship have not followed his lead. He thinks that, if one takes into consideration Sallust's Greek sources, one will find the probabilities of Sallustian authorship enhanced. We know, argues Professor Post, that Sallust was especially fond of introducing into his work paraphrases from the *Cyropaedia* and *Memorabilia* of Xenophon and from the *Epistles* and other works of Plato (e.g., in the *First Suasoria*, which is certainly by Sallust, the Roman historian makes obvious use of Plato *Epistle* 7); accordingly, if we can find passages in the *Second Suasoria* that are similarly derived, it would be at least good a priori evidence that Sallust wrote it. Professor Post, therefore, submits the *Second Suasoria* to a searching examination and arrives at the following results:

1. "Many of the commonplaces about wealth and pleasure and the rise and the downfall of nations have parallels in Plato, *Laws* and in Xenophon, *Cyropaedia*." But Professor Post admits that they are so vague that they do not prove much.

2. In the passage *sed mihi studium . . . cognitum habuerim* (ii. 1. 3), the first clause is a reminiscence of Plato *Ep.* 7. 324 B-C, and the remainder practically an epitome of Xenophon *Memorabilia* iii. 6.

3. The phrase *nullius potentia super leges erat* (ii. 5. 3) is probably a reminiscence of Plato *Ep.* 7. 334 C, and *Ep.* 8. 354 C: οἱκ ἄνθρωποι τύραννοι νόμων.

4. The passage *nam ubi bonus . . . animus ad voluptatem a vero deficit* (ii. 7. 6) "seems" to derive from Plato's account of the oligarchic man and his son in *Republic* vii.

5. The sentence *ergo in primis auctoritatem pecuniae demito* (ii. 7. 10) is fairly clear evidence of a use of Plato *Ep.* 8. 355 A-B; in *primis* echoes Plato's πάντων πρώτον.

6. The passage *ita coaequantur dignitate, pecunia, virtute anteire aliis alium properabit* (ii. 8. 2) derives from Plato *Menexenus* 238 D.

7. In ii. 13 the author of the *Second Suasoria* uses the Platonic device of putting an exhortation into the mouth of an apparition summoned for the purpose (cf. *Menexenus*, *ad fin.*; *Epistle* 8, *ad fin.*).

Now it seems to the writer that most of the examples adduced by Professor Post are neither very certain nor very obvious. The mere fact that hitherto they have not been recognized as deriving from Greek sources demonstrates

that; Professor Post's own language leads one to infer that only in the case of passages 2 and 6 is he himself certain about their ultimate Greek provenience.

Now, it occurs to the writer that the passages here adduced might be used to sustain a different line of argument. Professor Post argues that they indicate that the author of the *Second Suasoria* used the same Greek sources as Sallust did and that this fact tends to confirm Sallustian authorship. But, seeing that the resemblance of certain of these passages to their alleged sources is vague, might it not be the case that these reminiscences from Plato and Xenophon were not obtained at first hand? Professor Last, in the article to which allusion has been made, has pointed out that there are several passages in the *Second Suasoria* which seem to have been plagiarized from the *First*. Could it not be argued that the passages which Professor Post adduces are reminiscences by a forger of passages in Sallust, which in their turn are based on passages of Plato or Xenophon? For example, the first clause of ii. 1. 3, which Professor Post thinks a reminiscence of Plato *Ep.* 7. 324 B-C, could be a reminiscence of *Catilina* iii. 3, which is itself based on the Platonic passage. Some of the other passages with a Greek flavor might have been copied from passages in the lost *Historiae* of Sallust. Professor Post himself admits that some of the parallels between passages in the *Second Suasoria* and passages in the works of Plato and Xenophon are "vague"; he himself says that "the case for the *Second Suasoria* is not supported by any evidence so striking as the obvious use by the *First Suasoria* of Plato, *Epistle* 7." Could not this vagueness of resemblance, which induces Professor Post to use the cautious words "seems" and "probably" and "practically," be interpreted as meaning that a forger wrote the *Second Suasoria* and interlarded it with passages which he modeled on genuine Sallustian writings; that some of the genuine Sallustian passages he used were obtained from Greek literature; and that consequently his efforts, being one step farther removed from the Greek originals, are vague rather than recognizable derivations from the Greek?

The present writer is not saying that this is the case; he merely points out that it is possible to argue along these lines. It would appear to be better for supporters of the Sallustian authorship of the *Second Suasoria* not to adduce alleged Greek echoes in the pamphlet; such argument might prove a two-edged sword.

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#### SOSUS AND ARISTOPHANES

The mosaic of Sosus, known as the "Unswept Dining Hall," which he made for the dining-room of a Pergamene house<sup>1</sup> was sufficiently famous to have

<sup>1</sup> Pliny *NH*, xxxvi. 60. 184; for a Roman copy see Nogara, *I. Mos. Ant.*, Pls. 5-7.

contributed to Latin vocabulary the term *asarotum*. It represented a floor strewn with scraps of food which had fallen from the table. The theme has been criticized as indicating questionable taste on the part of the mosaicist, but it may have been prescribed by the owner of the house. Even so, the mosaic need not be regarded as an illustration of the extreme license of Hellenistic genre, for a fragment of *The Heroes* of Aristophanes<sup>2</sup> suggests that the choice of subject was dictated by household cult. This fragment is evidence of a prohibition that one should not taste of food fallen from the table because it belonged to the heroes: 'Αριστοφάνης δὲ τῶν ἡρώων φησὶν εἶναι τὰ πίπτοντα λέγων ἐν τοῖς "Ἡρώσι Μὴ γέεσθ' ἅττ' ἂν καταπέσῃ τῆς τραπέζης ἐντός. The same custom is mentioned by Athenaeus: τοῖς δὲ τετελευτηκόσι τῶν φίλων ἀπένεμον τὰ πίπτοντα τῆς τροφῆς ἀπὸ τῶν τραπέζων.<sup>3</sup> Diogenes, who quotes the passage from Aristophanes, adds two explanations of the prohibition. Either it was due to a desire for moderation in eating or it had something to do with the death of a person. The first is untenable but the second affords a real clue. The dead were heroized, and, since some of these heroes were buried in their houses the crumbs which fell from the table might properly be regarded as theirs. A comment by Servius<sup>4</sup> shows that such burial accounts for the worship of the Lares in the house: "apud maiores omnes in suis domibus sepeliebantur unde ortum est ut Lares colerentur in domibus." The Pergamene householder who wished to feed his Lares but objected to a dining-room littered with scraps resorted to a compromise both propitiatory and sanitary by depicting the bits of food in a mosaic pavement. In effect, he commissioned Sosus to borrow an idea from the artist who painted or carved a garland upon an altar as a permanent substitute for a real one.

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<sup>2</sup> Meineke, *Frag. com. Graec.*, I, 285.

<sup>3</sup> x, 427e; for striking but later parallels see Malten, Pauly-Wissowa, *RE*, Suppl. IV, p. 892.

<sup>4</sup> *Ad Aen.* vi. 152; cf. v. 64.

## BOOK REVIEWS

*P. Vergili Maronis Aeneidos: Liber Primus.* Edited with notes by the late ROBERT SEYMOUR CONWAY, LITT.D., F.B.A. Cambridge: University Press; New York: Macmillan Company, 1935. Pp. xiv+149. \$3.00.

Professor Conway, the well-known English classical scholar, died in September, 1933, leaving unfinished a new edition of the *Aeneid*, "a work planned originally as a further revision of the Conington-Nettleship edition of Virgil and one to which he had looked forward for many years." At the time of his death he had completed the commentary up to the end of Book i, and this is now published by his son, G. S. Conway, who with the assistance of Miss W. R. Walker, formerly Professor Conway's secretary, undertook to make "a careful though largely mechanical revision" of the notes, and to prepare three indexes. We are informed that "textual criticism and topographical and archaeological notes, which were to have been included in the book, are necessarily absent," as well as "the longer Introduction to the whole *Aeneid*." However, for Professor Conway's views on many topics we are referred to that scholar's numerous papers and books published between 1921 and 1933. The text, as printed, is based, curiously enough, on that of Sidgwick, but the notes usually give Conway's reasons for adopting a reading where there is a conflict of evidence, and in Book i the passages disputed are not numerous.

We are told that in line 2 Conway "was undecided between *Lauina* and *Lavinia*." In line 333, true to his love for M, Conway accepts the irregular *et uastis* for *uastis et*, but in 374, deserting M, he accepts the *componat* of PR. In 380 he follows Sabbadini in reading *Ioue magno* with R rather than the better-attested *Ioue summo*. In 427 he again sides with Sabbadini, reading *theatris*, though Servius supports the *theatri* of M and it is hard to see why the infant city should be building more than one theater. In 448 Conway rejects *nexae* in favor of *nizae*, which he calls "the better supported reading," though it is found in only one of the major MSS, viz., F, where a very early hand, *alio exemplari adhibito*, changed it to *nexae*. There is a similar instance in 550, where the *arma* of R  $\gamma$  is said to have "the best support," although M, which on the whole is far superior to R, has *arua*. Conway adopts *antiquae*, not *antiqua*, in 642; *longam*, not *longo* 703; and *it*, not *fit*, in 725. In each of these five instances he is following Sabbadini, but in 636 he parts company with the Italian scholar, and reads not *dii*, but *dei*. It is true that Gellius has said: "non dubium est quin *dii* scripserit pro *dei*," but Conway has pressed the claims of *dei* so convincingly that I now regret that his book did not precede my revision of the Loeb *Virgil*.



The notes, naturally enough, are very miscellaneous in character, and vary (unnecessarily so, I think) between quite elementary and extremely erudite comments, so much so that a reader must often wonder what kind of students Conway had in view when he was compiling his annotations. American teachers, at least, will find many of the grammatical notes quite superfluous, though they will welcome explanations of the full meaning of certain words, where the original signification has been lost or obscured. Thus, on line 21, it is noted that *superbus* betrays its literal force "towering," when applied to high-built towns like Tibur. The word *mons* (see n. on 105) means originally "something prominent," and its size depends on the context; "so in the derivatives in French, Italian and Welsh." In 126 *alto* is "the height of waters"; in 135 *praestat*, from *prae-stare*, has legal and even convivial associations (cf. "stand bail," "stand a drink"); in 185 *armenta* means properly "herds of plowing beasts"; in 355 *crudelis* is "blood-stained," being connected with *cruor*; in 519 *uenia*, having the same root as *uenus*, "charm," is properly like Gr. χάρις "graciousness"; in 535 *nimbosus* reminds the editor that adjectives in *-osus* have "a mainly bad sense in V. due to the origin of the suffix ('smelling of')"; in 537 *salum* is "tossing water (Eng. 'swell')"; in 607 *freta* is supposed "to contain a shorter form of the root of *feru-ere*, *de-fru-tum*." In 646 *stat*, "a slightly stronger form of assertion than *est*," reminds the editor that "in modern Italian, and still more completely in Fr. *étant*, *étais*, etc. (for [e] *stantem*), *stare* has become a verb of mere predication." In 688 *fallas* has "its orig. sense (cf. Gr. σ-φάλλειν, where the σ- is the remnant of a Prepn.) 'make to stumble, trip up, bewitch'"; and *uenenum* was originally "love-charm, philtre."

Conway is standing on less firm ground when he deals with literary criticism. Thus he is inclined to depreciate Lucretius, who, in the use of theme and variation, "indulged in almost *ad nauseam*" (see n. on l. 54), and who, in *De rerum natura* v. 945-52, is guilty of "a rather childish and labored (though not unbeautiful) attempt" to make sound and sense harmonize. This remark appears in the note on 166-69, which is one of the comparatively few notes devoted to the musical and metrical art of Virgil. Conway calls the *Culex* "V.'s schoolboy effort," but calls attention to the fact that in its 414 lines there occur almost as many examples of a double-dissyllabic ending as there are in the 4,755 lines of *Aeneid* i-vi (see n. on 719, which by the way repeats much of the n. on 444). As to the *Ciris*, Conway wisely regards it as non-Virgilian (see n. on 627). It is probably true that Shakespeare "knew this book well," but it is rather far-fetched to claim that he had in mind the beautiful picture of a mother's joy, found in 502, when he wrote in *Much Ado*, Act IV, Scene 1:

The idea of her life shall sweetly creep  
Into his study of imagination.



In the fields of linguistics and prehistory we expect to find Conway's annotations most suggestive and helpful, and, I may add, we are not disappointed. Thus, at 530, after noting that Virgil has changed a verse from Ennius not merely in meter but also in substance, Conway remarks that "Grai not merely recalls the etymol. of Hesperia, Ἑσπερος 'Abendland,' but places it (Serv.) in the range of Dido's experience, since Greek traders visited every port." Again, at 380, after speaking of the tradition that Dardanus was sprung from Corythus in Etruria and had founded the Phrygian race, Conway remarks that "the legend is borne out by the facts of language so far as they show the European origin of the Phrygians, whose surviving inscriptions prove that their speech was closely akin to Greek"—an interesting fact that may have a bearing on the Etruscan problem. At 569 we find the truly brilliant observation that Virgil "recognized what we call the Bronze Age as a real period just preceding recorded history, but he called it by the picturesque names of Gold and Saturn. That deity still conceals in his puzzling name and person a volume of prehistoric migrations."

As to Roman history proper, we cannot always agree with our editor. Thus, in interpreting the famous passage 286-90, he strives to convince us that Servius is right in recognizing a prophecy of Julius Caesar, rather than of Augustus. But this would rob the whole passage of unity, for everybody admits that the verses following, viz., 294 ff., refer to the closing of the temple of Janus in 29 B.C.

Conway makes no mention of English or American editions of Virgil later than Conington's, but he pays a tribute to Hale and Buck's *Latin Grammar* by selecting it, along with Roby's, for frequent reference, especially in regard to uses of the subjunctive mood. It is inconsistent to assign an exclamation mark to 101 and a question mark to 38, for the infinitive is used in the same way in both cases. It seems freakish to take *humo*, 193, and *cadis*, 195, as datives, or *uiderit*, 265, and *transierint*, 266, as subjunctive forms. And why, in such an edition, is it necessary to explain the so-called Latine accusative, 2; the descriptive ablative, 164; the datives of purpose, advantage, and with compound verbs, 425, 448, 627; together with causal and jussive subjunctives, 388, 645?

On the whole, however, and notwithstanding the fact that *Aeneid* i has been discussed and edited scores of times, it is extraordinary that Professor Conway has been able to produce so stimulating an edition. It is greatly to be regretted that death prevented him from continuing and completing his commentary upon all the books of the great epic.

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*Plutarchi Moralia*, Vol. II. Recensuerunt et emendaverunt W. NACHSTÄDT, W. SIEVEKING, J. B. TITCHENER. Leipzig: Teubner, 1935.

The second volume of the new Teubner *Moralia* has at last appeared. It

is not nearly so bad as the first volume,<sup>1</sup> but no better than the third volume, of which I have seen no adequate review. It appears to the present writer that what is most needed for a satisfactory edition of Plutarch is an accurate report of the readings of the manuscripts; and that report there seems to be little chance of obtaining. Critical acumen and conjectural restoration are well enough in their way, even if unfashionable; but we have had more critical brilliance in the past than we are like to have again. Even Mr. Pohlenz is usually no great improvement on Xylander or Meziriacus or Wytttenbach or Emperius or Cobet or Madvig. If the text of Plutarch is to be improved, and there is great need for improvement, we must know what the manuscripts read. Nothing is more pernicious than an edition which purports<sup>2</sup> to give the readings of some forty or more manuscripts, and in which not one, it is a fair inference, is collated accurately.

This was my impression upon reading Volumes I and III;<sup>3</sup> when I was asked to review Volume II, I prevailed upon my colleague, Mr. W. C. Helmbold, to aid me in reading such manuscript facsimiles as were available. Unfortunately, we had but two for these essays—E and B. E we have read for the *Aetia Romana et Graeca*, *Parallela minora*, *De fortuna Romanorum*, *De Alexandri fortuna*, *De gloria Atheniensium*; B for *De fortuna Romanorum*.

I am aware that E has been "discredited"; the question of its authority I shall not here debate. I merely ask these questions: If the editors do not believe in the value of E, why do they (particularly Mr. Titchener) disfigure their pages with its peculiar readings which they often adopt? Above all, why do they report these readings wrongly?

I proceed to illustrate:<sup>4</sup> fasc. 1] 278. 18 (265 E) τὰς not Pohlenz, but E; 286. 8 (269 A) γένει ἑλλην ἦν E; 287. 26 (269 E) τῆς is not in E, but was added by Mez.; 300. 13 (275 C) τὸ ante τῆς om. E: which τὸ does O omit?; 315. 9 (281 F) ζῶου E, like all the other manuscripts; 315. 20 (282 A) τῆς σελήνης E (*sicut viderat* Wytt.); 321. 3 (284 C) λέγεται E in text; 321. 6 τούτου E (*sicut* Wytt.); 324. 9 (285 F) δι' is not in E; 338. 5 (292 A) προκρίτους E (and O?); 341. 4 (293 C) καὶ habet E; 341. 17 τε E (Wytt.); 352. 22 (298 E) τούτω

<sup>1</sup> See the L. C. L. *Moralia*, II, vi, where the case is understated; contrast *Phil. Woch.* (1930), cols. 1105 ff.

<sup>2</sup> With modifications (new Teubner, I, xlii f.), which I have not the space to discuss.

<sup>3</sup> I take this occasion of pointing out a few errors in Vol. III: p. 30. 15 (396 F) there is a lacuna of about eighteen letters in both E and B after τὸ δὲ; 52. 17 (406 C) συναπονομένου both B and E (and cf. *Class. Rev.*, XLVIII. 26); 64. 10 (412 A) τῶν is not in E; 77. 25 (418 A) φωλεῶθ D; 81. 29 (419 F) τρέπονται πολλάκις δὲ καὶ λιμικοῖς D; 89. 3/4 καὶ . . . ἑλάττους om. D. I say nothing of the many errors in the attribution of conjectures.

<sup>4</sup> I say nothing of omissions of E's readings, even where these are, in my opinion, of great importance.

E (Wytt.); 354. 26 (299 D) *ταναγκρικήν* E; 363. 9 (303 C) *ἐκέλευσεν . . . ἐμβολόμενον* E.<sup>5</sup>

Fasc. 2] 7. 16 (306 D) *εὐωχουμένων* E; 12. 21 (307 E) *ἐν* E; 12. 24 after *ἐπεξενώθη* a lacuna of about six letters in E; 16. 13 (308 E) *αὐτὸν* E, not Duebner; 25. 21 (311 B) *τὴν* E, not Duebner; 27. 1 (311 D) *μετὰ δὲ* E; 31. 18 (313 A) *θεραπαινίδος* E, *sicut coni.* Pantazides; 31. 20 *ἡ* E; 35. 25 *θῶσιν* E; 36. 27 *ἦγετο εἰς θολίαν* E, not Duebner; 47. 1 (317 C) *ἄπταιστον* B (not -ως); 49. 8 *οὐ μὲν γὰρ ἀπειθῆς* superscript in B, *sicut coni.* Reiske; 51. 1 *κατὰ* E, *sicut coni.* Reiske; 57. 10 (320 D) *ὑπέσχεον* B and E;<sup>6</sup> 87. 19 (331 C) *οἶει* in marg. E, *sicut coni.* Xyl.; 130. 23 (348 F) *εἰπεῖν* E.

These, and some forty more, are serious misstatements of the readings of but one manuscript in the *apparatus criticus* of fasc. 1] pp. 273-366 and fasc. 2 entire; and of another manuscript in fasc. 2] pp. 43-74. Of interesting readings omitted by the editors or unknown to them I have no space to tell the tale.<sup>7</sup>

The same type of error, as I have had occasion to remark before, is exhibited in the editors' overwhelming carelessness in the attribution of conjectures. I shall illustrate briefly: fasc. 1] 276. 11, not Naber, but Abresch; 283. 23/4, not Po., but Herw. (1890) and Pant. (1897/8); 308. 18, not Po., but Hatz.; 315. 1, not Hu., but Ha.; 316. 17, not Duebn., but an early correction in the Voss., according to Wytt.; 321. 19, not Hu., but Ha.; 347. 9, not Duebn., but Wytt.; 348. 2, not Duebn., but Mez.

Fasc. 2] 28. 15, not Sch., but Herw.; 41. 20, not Kron., but Herw. (*Mnem.* xviii. 382); 69. 14, not Bern., but Madvig (*Adv.* i. 628); 82. 2, not Duebn., but Wytt.; 103. 9, not Po., but Blümner (*Hermes* li. 46); 104. 17, not Wytt., but Mez.; 110. 18/9, not Bern., but Abresch; 122. 11, not Wil., but Herw.; 126. 9, not Na., but Madvig.

Fasc. 3] 28. 24, not Po., but Madvig; 40. 8/9, not Si., but Madvig. The editors, not quite always, ignore the credit due to scholars, who have discovered the truth by conjecture, when their surmises have later been confirmed by new manuscripts.

Inconsistencies are rife: the Angelicanus appears as Ang. and Angel. (also as Aug. by misprint); Pantazides appears as Pant., Pantaz., and Pantazides; Stephanus as St. and Steph.; Hatzidakis as Hz. and Hatz. We read now *καπιτωλ-* (1] 304. 24), now *καπετωλ-* (324. 7, *al.*); *Σεστυλίας* (328.

<sup>5</sup> Mr. Titchener's dissertation of 1924 gives far more and much more reliable information regarding E, at least, than his edition of 1934. What has happened to his proof-reading? Or has the Teubnerian *Übertieferungsgeschichte* undone him?

<sup>6</sup> There are some thirty cases of minor agreements of B and E in *De fort. Rom.* alone not mentioned in the *app. crit.*, though I lay no particular stress on this.

<sup>7</sup> I cannot at this time check the results of Mr. Sieveking's collations in fasc. 3; they may be perfectly correct. He has certainly far fewer MSS to deal with.

24), but Σέξτος (330. 27, *al.*); Οἰέppου (100. 9), but Βέppου (101. 11). There are scores of others of the type of οἰκτίρω, μείγνυμι, θάλαττα, φυλάττω; but these are partially defended in I, xlii f.<sup>8</sup>

Misprints, in comparison with positive errors, are strikingly few.<sup>9</sup> As might be expected and must assuredly be condoned, since nothing short of an army of trained workers could set it right, the parallel passages exhibit not a few errors and omissions. With these I forbear to trouble the reader; they are unavoidable.

Since these things are so, I feel compelled to reaffirm my opinion of 1928: The new Teubner edition continues to be a great disappointment and is still quite unsatisfactory. We shall surely be justified in mistrusting the editors' account of the relationship of the manuscripts when their collations are so patently incorrect. But in saying this I have no wish to bring about a war of personalities, an *unschöner Streit*,<sup>10</sup> such as disfigured the unfortunate Bernardakis' productions. If my criticisms are just, they should bring about repentance; if they are unjust, they may be ignored. My only purpose in pointing out the errors of this edition to those who are not in a position to detect them is that the fourth and succeeding volumes of the series may be free from such a mass of error; that the editors shall not make lofty pretensions which they cannot, or are unwilling to, fulfil; that at long last we may obtain an *apparatus criticus* worthy of the name. I care nothing how the editors emend and restore, subtract and modify; Bernardakis, to say nothing of greater men, was as good at this game as they are: what I wish to know are the facts.<sup>11</sup>

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December, 1935

*Zenon Papyri*, Vol. I. Edited by WILLIAM LINN WESTERMANN and ELIZABETH SAYRE HASENOEHRL. New York: Columbia University Press, 1934. Pp. 177. \$6.00.

Some of the records of the Zenon archives have come into the possession of Columbia University, and in this first volume fifty-eight documents, of

<sup>8</sup> It is amusing to observe that a conjecture adopted at 1] 250. 17 produces *hiatus*!, as the editors call it.

<sup>9</sup> Some are 1] 27. 7 *app.*; 325. 26; 329. 25; 335. 25; 350. 3; 355. 16 (*λεπίως τοῦ om.*); 365. 17; also 353. 26?

<sup>10</sup> J. Schönemann, *Phil. Woch.* (1933), col. 344. I regret that Mr. Schönemann has not changed his views about my nationality (*Phil. Woch.* [1932] col. 130). I also regret that I cannot greet the new Teubner enterprise with a hymn of praise, as Mr. Schönemann wishes me to do.

<sup>11</sup> After the lamented death of Mr. Babbitt on September 21, 1935, this review was completed and edited by his colleague, Mr. Helmbold. Mr. Helmbold makes himself responsible, in particular, for all statements of fact contained in this review.

which nine had been previously published, are presented. Their chief importance is their contribution to economic history. A record of caravan transportation (No. 2) shows that Apollonius was engaged in developing trade with Palestine and his caravans ranged far afield even as far north as Sidon. It is somewhat surprising to find caravans from Egypt at Sidon, and, as the editors suggest, Apollonius was evidently attempting to cut into the monopoly of the Nabataeans. The shipment of grain from Sidon to Galilee is rather tantalizing. Elsewhere in the record the destination of the caravans is given with exactness, but Galilee is a large country and the actual destination of the grain shipped to Galilee and beyond is not indicated. One suspects that the vagueness of the record may portend some movement of troops in this region for which these camels were requisitioned to provide supplies. Or else the country may be suffering from local famine, and the caravan leaders were at liberty to go wherever they could get the best price. Presumably the cost of moving wheat from Sidon to Galilee was 24 dr. per camel. In the Fayum wheat was selling at 2 dr. per artaba about this time (No. 5), and this same wheat delivered in Galilee must have cost from 8-10 dr. per artaba. Evidently Galilee had a dearth of supplies, else the grain would have been secured from local sources. In the caravans moving from Gaza to Egypt (134 miles) each camel earned 25 dr. per load while on the return journey the rate was 16 dr. per camel. From this disparity in rates we may infer imports to Egypt along this particular route were greater than the exports.

The account of a pilot or captain (No. 43) gives some interesting details about prices. A towrope cost 3 ob., an anchor 3 dr., oars and punt pole  $2\frac{1}{2}$  dr., rushes for calking 2 dr.  $5\frac{1}{2}$  ob., and tallow used in launching the craft 2 ob. The captain received 15 dr. as wages for Mechir and 8 dr. in Phamenoth. The editors observe that the pay for Mechir probably included that of the crew since another document from these archives gives the wages of the captain as 10 dr. and those of the crew as  $7\frac{1}{2}$  dr. monthly. The value of a goose given for a festival was 5 dr. Geese were evidently costly. A sale in Roman times shows that they were valued at 40 dr. each (PSI 961).

Workmen on the estate of Apollonius received 2 dr. 2 ob. a month in cash, an allowance of 14 dr. a year for clothing, and monthly rations of wheat and oil (Nos. 5, 31). Donkey drivers received 1 dr.  $1\frac{1}{2}$  ob. a month (No. 20). In the latter case the editors suggest that this was paid for a special contract rather than as a regular monthly wage. However, the scale is not far below that indicated for ordinary workmen, and boys were frequently employed in this capacity. A scribe received 15 copper dr. a month although the usual salary of secretaries was 5 dr. a month with keep. Evidently the economic security of the average workman at this time was precarious. Wheat cost 2 dr. per artaba, and food alone would cost 24 dr. a year without any allowance for wine, or oil, or other necessities. The average workman earned 28 dr. a year in addition to his keep, and it is evident that all adult members of his household must also have been wage-earners in order to exist.

Apollonius assisted his tenants on leaseholds of virgin land by lending them money without interest to assist them in clearing the land. These loans were advanced at the fixed rate of 4 ob. per aroura. He himself hired men for the same purpose at a contract rate of  $2\frac{1}{2}$  dr. per aroura. With the scale of wages at this time it evidently took a man about a month to clear a single aroura (Nos. 22-28).

Wine was sold from government stores to retail dealers at 6 dr. for a six-chous measure. A chous of castor oil cost 4 dr. Shoes were sold at 3 copper ob. A chlamys, evidently of good quality, was worth 37 dr.  $3\frac{1}{2}$  ob. Mattresses with purple borders cost 31 dr. 2 ob. each. These commodities were evidently luxuries for the ordinary peasant.

The consumption of paper at the office of Apollonius was surprisingly large, for 434 rolls were distributed in thirty-three days. Apparently the cost was  $3\frac{1}{2}$  ob. for a roll. In the reign of Claudius the record office at Tebtynis paid 4 dr. per roll. If the rolls were the same size in both periods, it is evident that the cost of paper had risen considerably even if the depreciated value of the Roman drachma is taken into account.

One of the new documents shows that the revenues set aside for the cult of Arsinoë Philadelphus had been diverted to secular uses by 250 B.C. (No. 55). Another is a letter from the Caunians to Zenon asking for his support in a proposal which they were making to Apollonius regarding their native city. Evidently the latter had considerable influence in the administration of the Carian dependencies.

These documents are an important contribution to our knowledge of the activities of Apollonius and to the economic history of his times, and they have been edited with the thoroughness characteristic of the Columbia school of papyrology. No item of significance has been omitted in the elaborate commentary.

ALLAN CHESTER JOHNSON

Princeton University

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*Actors' Interpolations in Greek Tragedy Studied with Special Reference to Euripides' Iphigeneia in Aulis.* By DENYS L. PAGE. Oxford: Clarendon Press, 1934. Pp. xi+228. \$3.50.

In several instances we are expressly informed by ancient sources that certain passages in Greek tragedy were due to interpolations by actors or others, and many others have been suspected by the acumen of modern scholars. Such interpolations seem especially plausible in the case of the *Iphigeneia at Aulis*, since it was produced posthumously and since the condition of the text suggests that the play lacks final revision. Page considers that the discussion of the general problem has suffered from the fact that there has been no generalized treatment but only the opinion of individual editors upon isolated passages, and in particular because there has been little attempt to determine by whom and at what period and for what purpose the interpolations were



made. Moreover, to "end with the detection of interpolations" is to stop where one's study

ought surely to begin again . . . for it is probable that those interpolations will teach him something of the history of such old tragedies as were popular in the fourth century and later, and dissipate at least a part of the darkness which encompasses all tragic texts for nearly two hundred years [p. x].

By a histrionic interpolation the author means

an alteration made by or for actors in the text of a play or affecting the text of a play. . . . The alterations are of various kinds—of words and verses, of punctuation and distribution of lines, of stage-management, the removal and insertion of verses, and so forth. It should not seem too great a liberty of language to include, e.g., cuts, changes of single words, and redistribution of verses among speakers, in the definition of interpolation [p. xi].

After a preliminary section entitled "The Scope of the Inquiry" (pp. ix–xi), the work is divided into two parts, "Introduction" (pp. 1–121) and "Interpolations in *Iphigenia in Aulis*" (pp. 122–216). Each of these is divided into three or four chapters, and there are also excursuses, addenda, and indexes. In the first chapter of Part I, *τὰ εἰκότα* are used in the attempt to discover when in the history of the text interpolations were most likely to be made. In the next three chapters the author applies these results to recorded interpolations in several plays other than the *Iphigenia Aul.* and tries to deduce "Some Characteristics of Histrionic Interpolation," which is the title of chap. iv. Then, in Part II, the *Iphigenia* is studied in the light of all these deductions.

It may be said at once that Part I does not provide an adequate instrument for the purpose for which it is used in Part II.<sup>1</sup> The book does have a certain value, however, for the author's considered judgment upon passages which have vexed so many editors.

More concretely, the author leans too much upon Wilamowitz' *Einleitung in die griechische Tragödie*,<sup>2</sup> which was valuable in its day but was published nearly half a century ago! He believes that Aristophanes of Byzantium brought out an edition of Euripides in volumes of eight (or nine) plays each, assembled principally on an alphabetical basis, and finds evidence for this view (pp. 3 f.) in a list of titles appearing in *Corpus inscriptionum Graecarum*, II, 992 and in the sequence of plays in the Laurentian manuscript. In dis-

<sup>1</sup> The author himself acknowledges this on p. 120: "Now it may be asked whether we have found principles to guide us in the future; whether, if a passage is newly suspected, we can test its quality by means of principles. We have not, and we cannot." Yet on the next page he adds: ". . . hence we have a store of facts to assist us in our treatment of interpolations which we have not yet considered."

<sup>2</sup> Cf. p. 1, n. 1: "Much of this chapter is based on Wilamowitz. . . . Familiarity with the arguments there employed is assumed."



cussing the inscription he interprets ΑΑΑΙ and ΑΦΙΑΝ [ΑΙ as titles; but there is no evidence for Euripidean plays with such names, and these words are now believed to refer to Attic demes (cf. the new *editio minor* of the *Corpus* [known as *Inscriptiones Graecae*], No. 2363). Moreover, the author ought to have dealt with the fact that there was another edition of Greek plays designated by numbers. In *Classical Philology*, V (1910), 1-18, I maintained that these numbers were chronological in sequence for each author so far as the volumes were represented in the Alexandrian library. The evidence was not quite cogent at that time but became so with the discovery of an additional numeral (cf. my *Greek Theater and Its Drama*, pp. 330-37). So well-authenticated an edition surely should have been included in the discussion of Part I, chap. i.

The title-page describes the author as "student and tutor of Christ Church, Oxford." Evidently he is a young man who is to be congratulated upon producing so scholarly a work. But the Clarendon Press reader should have protected him from such inconsistencies as Aelian, Aeschylus, Aischines, Aristoxenos, Athenaeus, etc. display, or such a monstrosity as Lukourgos; from referring to books of ancient authors now by Roman numerals, now by Arabic numbers, and even by Greek letters (p. 15, n. 5); and from the erratic use of punctuation and italics. On page 17 *θεάτρον* is incorrectly accented.

ROY C. FLICKINGER

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*Harvard Studies in Classical Philology*, Vol. XLV. Cambridge: Harvard University Press, 1934.

The first article in the volume (C. T. Harrison, "Ancient Atomists and English Literature of the Seventeenth Century") is also the longest (79 pages). It of course deals chiefly with Lucretius. His influence began in England in the middle of the seventeenth century, later than in France and Italy. The account is a general one, dealing with ideas and movements rather than with detailed discussion of parallel passages. It is a valuable contribution in a little-worked field.

Mason Hammond summarizes the life of Corbulo and gives him credit for Nero's eastern policy.

J. S. Newberry ("The Prehistory of the Alphabet") has a partly new but not convincing account of the origin of the alphabet. He does not think that the Greek alphabet came from the Phoenician but that both are descended from a common ancestor—Cretan. The Cretan script itself is said to be derived from Sumerian. Egyptian and Chinese writing are attributed to the same source. Until the Cretan script is deciphered to the satisfaction of the scholarly world, all identification of its signs with Greek (Phoenician) characters is hazardous. If Newberry is right, it ought to be easy to read Cretan, for he has given values to many of its signs. The statement that the Minoan

signs "are much closer to the letters of the archaic Greek dialect inscriptions than are the Phoenician characters of the thirteenth century" is disproved by Newberry's own table. The Philistines, coming from Crete, are said to have introduced the alphabet into Asia. There are, of course, obvious similarities between Minoan signs and Greek letters, as there are between Minoan and Egyptian. The explanation would seem to be that Minoan is derived from Egyptian or that both come from the same source.

Since Greek bears undeniable resemblances to early Semitic writing, Newberry ingeniously explains that the Greeks changed the shapes of some letters to imitate the Phoenician in the eighth century. But a study of the actual remains of the earliest Semitic and Greek alphabets makes this view untenable (see my article in *AJA*, XXXVIII [1934], 359, which unfortunately appeared too late for Newberry to use).

Newberry supposes that the Greeks did not develop the vowels but inherited them from the Minoans, who in turn got them from the Sumerian. Not only that, but even upsilon and the supplementary letters phi, chi, psi (and omega, to boot) go back to Sumerian. Thus Newberry says that omega comes from the corner-wedge,  $\upsilon$ , and the man sign, MA, ME, with the value  $\upsilon$ -MU, etc. The sign represents the phallus and the womb. For those only slightly familiar with the Greek alphabet and its history it is not necessary to go into detail on the supposed derivation of the other letters and their names from Sumerian.

J. J. H. Savage describes a number of manuscripts of Servius' commentary of Virgil and comments briefly on the classes. G. B. Waldrop gives some readings of these manuscripts and presents a *stemma*. These articles, of course, are in preparation for the monumental edition of Servius being prepared under the direction of E. K. Rand. In the final paper C. F. Edson, Jr., deals with the attempts of the Antigonids to restore Alexander's empire.

B. L. ULLMAN

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*P. Cornelii Taciti libri qui supersunt.* Recognovit CAROLUS HALM. Post GEORGIUM ANDRESEN denuo curavit ERICUS KÖSTERMANN. Tom. I, fasc. 1: *Libri ab excessu Divi Augusti I-VI*; Tom. II, fasc. 1: *Historiarum libri*. Leipzig: Teubner, 1934-35.

Andresen's revision of the Halm edition of Tacitus' works showed a distinct advance on the four previous editions; and what Andresen did for the fifth edition of Halm in 1913-14, Köstermann has now done for Andresen. For in spite of the general excellence of the latter's work it was marred by certain defects, and these Köstermann has succeeded in removing. He has made a careful and independent study of the manuscript readings and the result is a much-improved text. Nor are his changes entirely due to his choice of

variant readings found in the manuscripts. He has introduced also a number of emendations suggested by different scholars in the periodical literature of the last thirty years. He has, however, been cautious and has not adopted any emendation without good reason. The references to the new literature constitute the principal element of change in the critical apparatus, which in other respects follows the Halm-Andresen closely and has been kept within the strict limits usual in Teubner text editions. Köstermann's orthography differs at times from that of Andresen, and in some chapters he has used a different paragraph division.

GORDON J. LAING

University of Chicago

*Dura Studies.* By JOTHAM JOHNSON. Philadelphia, 1932.

In this University of Pennsylvania thesis we have three studies, by-products of the Dura-Europus excavations. One brilliantly establishes the intercalation of an additional month, some time before the end of the first century A.D., to bring the Babylonian and Macedonian luni-solar calendars into closer agreement with the solar year. The Palmyrene calendar, though without the use of the intercalary month, has the same shift. With the aid of such tables as those prepared by the reviewer it is now possible to turn Macedonian dates through the exactly equivalent Babylonian into standard dates of the Christian reckoning.

A less happy suggestion, that the shift took place 139-38 B.C., was already negated by the fact that the last Babylonian documents from just before the Christian Era show no evidence for the shift. McDowell has shown that the change had taken place by 47-46 A.D., and notes that "the group [of coins] constitutes the sole occurrence of month dates on Parthian royal bronze."<sup>1</sup> To the reviewer there seems but one plausible explanation of this unprecedented procedure—that the additional month had been intercalated in A.D. 45, and that the months in their new order were placed on the copper coins that attention to the shift might be directed to those who would not use the silver coins, on which months were sometimes placed. Naturally, the table of Johnson should be corrected between 139 B.C. and A.D. 45. A second study works out anew the family tree of the hereditary *strategos* and examines the family life; a third is a revision of Parchment II.

A. T. OLMSTEAD

*Die Satrapieneinteilung in Syrien und in Zweistromlande von 520-320.* Von OSCAR LEUZE. ("Schriften der Königsberger Gelehrten Gesellschaft," Band XI, Heft 4 [1935].) Halle: Max Niemeyer Verlag. Pp. x+476.

This solid work, unfortunately posthumous, is a definite contribution to the difficult problem of the satrapial organization of the Achaemenid Persians,

<sup>1</sup> R. H. McDowell, *Coins from Seleucia on the Tigris* (1935), pp. 151 ff.

so important for both Greek and biblical history. Many of the suggestions are worthy of serious consideration. Unfortunately, until the new material from Persepolis is published and analyzed, until the abundant Babylonian tablets from the same period are given similar treatment, the full story of Persian administration cannot be told.

A. T. OLMSTEAD

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*Yale Classical Studies*, Vol. III. Edited by AUSTIN M. HARMON. New Haven: Yale University Press, 1932. Pp. 208. \$2.50.

"Seleucid Babylonia: Bullae and Seals of Clay with Greek Inscriptions," by M. Rostovtzeff, forms the major part of the volume. No better example could be found of Rostovtzeff's uncanny ability to bring together the most apparently unimportant objects and from them create significant history. As a result, *bullae* have already become an important source in historical investigation.

*Bullae* in this sense are lumps of clay or bitumen affixed to parchments or papyri now lost but still retaining the impress of the seals or stamps of the certifying officials, contracting parties, or witnesses. Private seals may be purely Greek, half Greek, or wholly native in ornament; it is significant that the sealings from Seleucia show little oriental influence as contrasted with the more oriental Uruk. Rostovtzeff rightly points out that further study of these seals will give important results for the history of art, religion, and culture; when to this we add a careful study of the sealings on the numerous cuneiform tablets from the Seleucid period, of which few have been published and still fewer translated, important results may be expected.

The special purpose of this study is to throw light on Seleucid administration. It is a welcome light, for the data here secured refer primarily to the higher administration, of which we catch but glimpses in the published cuneiform tablets. Sealings with portrait heads of kings and queens may be considered definitely official—almost as certainly those with the royal divinities or administrative motifs. There can be no doubt that the stamps with Greek inscriptions are connected with the local record office, and fortunately they are dated. Uruk generally furnishes the fact that they come from the record office of Orchoi and that they are sales taxes; Seleucia furnishes many examples of the salt tax; and both, examples of the slave sale tax. From his wide wealth of knowledge Rostovtzeff illuminates each point of detail. Since the work was published, the Seleucid *bullae* have been given fuller publication by R. H. McDowell in *Stamped and Inscribed Objects from Seleucia on the Tigris* (1935).

These *bullae* are a welcome addition to the few and scattered literary sources for Seleucid administration. But what we especially need is the publication of all known cuneiform tablets from the Seleucid period, translation

of those already published, and careful investigation. Parallels from the Greek inscriptions and papyri are welcome, but we should never forget that the Hellenistic administration and indeed the basal economic life were taken with little change from the older Orient. The salt tax, now well known from Seleucia, can be traced to at least the Chaldaean period. The general tenor of the Seleucid tablets is fundamentally the same as those of the Achaemenids. Under the Seleucids we find, for example, the temple council of the earlier periods still with some local administrative functions; and one tablet gives us the names of the twenty-five members with official titles and seals, all purely oriental. The taxes remain the same in name, as a rule, but there is one significant exception. McDowell would assign the beginnings of the slave tax to Antiochus I; Rostovtzeff is doubtful. A number of slave sales on early Seleucid tablets are exactly like similar Persian sales, but on the last known, from the reign of Antiochus I, there is an additional seal of a royal official.

"A Neo-Pythagorean Source in Philo Judaeus," by Erwin R. Goodenough, and "Agriculture in the Life of Pompeii," by John Day, close an unusually interesting volume of these studies.

A. T. OLMSTEAD

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*Diodorus of Sicily.* With an English translation by C. H. OLDFATHER. ("Loeb Classical Library.") Vol. I: pp. xxvii+470; Vol. II: pp. x+539. New York: G. P. Putnam's Sons, 1933, 1935.

With these volumes Professor Oldfather begins his ten-volume edition of Diodorus' *Library of History*, a very welcome addition to the series of Loeb texts. In Volume I he gives the text and translation of Book i and chapters 1-34 of Book ii. This covers Diodorus' own Introduction in which he sets forth his philosophy of history, the discussion of the origins of the universe and of life, and the treatment of Ancient Egypt, Assyria, Babylon, and the Median Empire. Volume II contains the rest of Book ii, all of Book iii, and the first fifty-eight chapters of Book iv. Here we find the descriptions of India; of the Scythians, Amazons, and Hyperboreans; of Syria, Palestine, and Arabia; of the mythical island in the southern ocean; of the Ethiopians and the peoples of the coasts of the Red Sea and the Arabian peninsula; and, finally, legends of Libyan and Greek mythology, including a detailed account of the adventures of Herakles. In his general Introduction the editor discusses the life of Diodorus, the period during which he was at work on his history (from before 59 to 30 B.C.), and the scope and character of the undertaking, which was planned originally to come down to 45 B.C. He also defends Diodorus against the charge that he was a mere "scissors and paste" historiographer, although, in the reviewer's opinion, the portion of the *Library* here presented gives the impression that the author was but an indifferent observer, very unfortunate in the selection of his sources, and endowed with

little critical acumen. The Introduction to Volume II presents what is known about the sources used by Diodorus for the parts of his work here in question. The text is based on the Vogel-Fischer edition of 1888 ff. in the Teubner series. In the translation the editor has combined accuracy with readability—no small achievement in view of the general dullness of style of the original. In this connection, however, one may venture the suggestion that *δαίς* in i. 36. 2 should be rendered "mud" or "silt" rather than "slime"; and that *πυρῆν* in iii. 45. 7 might, in conformity with the translator's own note, be given the meaning of "a fruit stone" instead of "the stone of fruit." The footnotes are appropriate and adequate except for the omission of any reference to the recent work on the problem of Lake Moeris which has definitely disposed of the reservoir theory. However, no system of notes possible under the plan of the series could meet the real need of an up-to-date commentary on Diodorus. Typographical errors are very few, but in the quotation from Herodotus ii. 177 in Volume I, page 224, note 2, *ζόνη* should be *ζώνη*. Partial Indexes of proper names (a complete one is promised for the last volume) and maps of the appropriate parts of Africa and Asia complete the volumes.

A. E. R. BOAK

University of Michigan

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*Mediaeval "Artes praedicandi": A Hand-List.* By HARRY CAPLAN. ("Cornell Studies in Classical Philology," Vol. XXIV.) Ithaca, N.Y.: Cornell University Press; London: Humphrey Milford, 1934. Pp. 52. \$1.50.

*Mediaeval "Artes praedicandi": A Supplementary Hand-List.* By HARRY CAPLAN. ("Cornell Studies in Classical Philology," Vol. XXV.) Ithaca, N.Y.: Cornell University Press; London: Humphrey Milford, 1936. Pp. x+36. \$1.00.

Professor Caplan's studies in the medieval theory of preaching and the extent of its dependence on classical rhetoric have led him to publish these lists of tracts composed for the preacher during the thirteenth, fourteenth, and fifteenth centuries. The order of the *Hand-List* is as follows: "*Initia* of Unpublished Manuscripts"; "*Initia* of Tracts for Which Manuscripts Are Lacking"; "Tracts in Manuscripts of Which the *Initia* Have Not Been Obtained"; "Tracts for Which Both Manuscripts and *Initia* Are Lacking"; and "Published Tracts." Altogether there are 229 items, of which the first and most important list contains 183. This list, arranged alphabetically, gives the *incipit* of the tract, its title and the name of the author (if known), the manuscripts containing the tract (with folio numbers), and often further enlightening comment. The *Supplementary Hand-List*, in which in the main the same procedure is observed, offers new, additional information and frequently corroborates or corrects assumptions tentatively made in the first publication.



One could hardly demand personal examination of all the manuscripts listed. The author assures us that he has seen the manuscripts of the *Hand-List* "in large part" and "most of the manuscripts" of the supplement. Erroneous items that remain should therefore be attributed to faulty entries in catalogues of manuscripts and to other sources of knowledge gained at second hand. Errors in the supplementary lists seem to be few. But there still are several misprints and discrepancies in the earlier lists that have not been cleared up in the supplement; most of them, however, are not serious. Also, despite the author's explanatory warnings, the significance of some of the items is not readily apparent, particularly in cases of cross-reference. For example, one is puzzled on noting in the supplement that an evidently identical tract in a Seville manuscript is cited under No. 16 as revealing the author's name, and under No. 115 as being anonymous. But usually, when the same tract is given under two items, the reader, if not informed, can assume that in one of the two instances the *incipit* is false.

Confident that his lists offer a large majority of the manuscripts on the art of preaching to be found in the libraries of Europe, Professor Caplan hopes that they will stimulate interest in this cultural activity of the Middle Ages, so that a number of the tracts will be published. Several scholars have already announced to him plans for engaging themselves in this undertaking.

EDGAR C. REINKE

University of Chicago

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*Notes on the Text of Lucretius.* By N. H. ROMANES. Chiswick Press, 1934.

*Further Notes on Lucretius.* By N. H. ROMANES. Chiswick Press, 1935.

In the first of these attractively printed volumes Mr. Romanes discusses the textual problems in fifty-nine passages of Lucretius and sets forth various remedies and suggestions. The second volume contains, in a somewhat less systematic manner than the first, notes on 249 textual difficulties, wherein the author propounds a new emendation, appraises the work of other editors and scholars, or supplements the statements made in the first volume. An Appendix, covering pages 79-84, supplies a partial criticism of Martin's new Teubner edition, with considerable emphasis laid upon its anticipation or confirmation of Mr. Romanes' own suggestions and interpretations.

In view of the labor which went into the study of Lucretius and into the writing of these books, it may appear ungracious to say that most of the notes seem, at least to the present reviewer, wholly inconclusive. And yet in neither of the volumes is any suggestion which seems at all compelling, and few even plausible. Many, indeed, seem the product of perverse ingenuity. Among the proposed changes which may possibly be right should be included the notes on iii. 962 (cf. *Notes on the Text of Lucretius*, p. 26), where Mr. Romanes would



read: "aequo animoque agendum ac annis concede; necessest"; on v. 737 (*ibid.*, p. 42), where he suggests: "it ver, it Venus et Veneris praenuntius ante"; and on vi. 1199 (*ibid.*, p. 60), where he emends thus: "quorum si quis item vitarat funera leti." In *Further Notes on Lucretius* the following proposals deserve to be considered. In i. 149 (cf. *ibid.*, p. 8) Mr. Romanes would transpose *principium* and *cuius*, although he is grossly in error in asserting that *cuius* "as a dissyllable" occurs "hundreds of times," unless he means the statement to apply to all Latin poetry and not solely, as the context implies, to Lucretius. In the much-disputed verse, iii. 84, he would read (*ibid.*, p. 29): "rumpere, qui in summa pietatem euertere suadet." His suggestion (*ibid.*, p. 41) that iv. 397-99 should be placed after iv. 390 has a certain plausibility and would clear up in part the syntactical impasse. In his note on iv. 1039 (*ibid.*, p. 47) the author makes what seems to the reviewer his best emendation: "namque alias alia ut res commovet atque lacessit."

The two volumes, however, must be considered in their entirety. When he wrote *Notes on the Text of Lucretius*, Mr. Romanes seems to have been unfamiliar with Diels's edition. Even in *Further Notes on Lucretius* he pays no heed to Diels's discussion of the textual tradition or of the orthographic problems. More specifically, one may seriously question the view (cf. *Notes on the Text of Lucretius*, p. 14) that "'consanguineum' here [i.e., iii. 73] is, for some reason, a stock example of harsh contraction in the gen. plur." *Tortare*, despite the assurance (*ibid.*, p. 25) that it is a "Lucretian word," does not appear to occur as a transitive verb in Lucretius. (In iii. 661 *tortari* seems to be deponent or at least middle in force.) In view of Propertius iv. 1. 11 it is hardly fair to dismiss (*ibid.*, pp. 26-27; *Further Notes on Lucretius*, pp. 37-38) *patrum* in iv. 79 as "sheer nonsense." Nor is one obliged to admit (cf. *Notes on the Text of Lucretius*, p. 28) that the subjunctives in iv. 194 "are without point." The introduction of *bure* (*ibid.*, pp. 39-40) into the sufficiently explicit passage at iv. 1035, even if the use of *buris* in an erotic sense were adequately attested, seems utterly fantastic. In view of ii. 126, 438; v. 502, 504, one can hardly be so confident as Mr. Romanes is (cf. *Further Notes on Lucretius*, p. 16) that "*tubare* intrans. . . . is not Lucretian." I find it difficult to believe (*ibid.*, p. 45) that *emittitur* in iv. 504, 795 means "is passing away."

Except for the plausible suggestions noted above, these two volumes, I regret to say, are more important as indicating an interest in Lucretius than as contributing in any appreciable degree to the elucidation of the textual problems still awaiting solution.

STANLEY BARNEY SMITH

Bowdoin College

Dion: *Die Platonische Staatsgründung in Sizilien*. Von RENATA VON SCHELIHA. Leipzig: Dieterich'sche Verlagsbuchhandlung, 1934.

This discussion of Plato's "return to the cave," his attempt to play the part of philosopher-ruler in Sicily, is initiated by two serious faults of

method. The author is entirely uncritical in her use of source material; the genuineness of all the Platonic epistles is accepted without question. This assumption is explained in a footnote (p. 154, n. 11), "Der Darstellung Dions habe ich Platons Briefe als unantastbar richtige Quelle zugrunde gelegt. Daneben den ausführlichsten Bericht, Plutarch, Diodor Buch XVI und Cornelius Nepos. Da diese Quellen meist und in den wichtigsten Fragen übereinstimmen, gebe ich im einzelnen keine Stellennachweise." The reviewer can comment only with a gigantic exclamation point.

More serious than this is the author's concept of historicity. There is no attempt to penetrate beyond a historical fact to the social forces which create it and from which it derives its significance. For example (pp. 2 and 3), it is argued that the hoary antiquity of cities in Greece proper gave their laws and customs a permanence that was lacking in the Sicilian cities, which were recent foundations, not natural growths. Hence they were plastic material for political innovators (Pythagoras, Taleneus, Charondas, Empedocles); and so it was to Sicily that Plato went to try his experiments. Such an attitude to historical forces is breath-taking in its naïveté. There are numerous examples in every chapter. Under the influence of a distorted concept of the meaning of historical forces, simple historical facts are colored or wrested to suit the purpose in hand. The picture presented lacks shades and subtleties of tone; Plato the renewer of Hellas; Dion the doer—his *Schönheit, Ruhm, einsamer Kampf und tragischer Untergang* are melodrama, not history.

Nor is it hard to detect the source of this distortion. A subtler mind might have made the task more difficult: her naïve enthusiasm leaves it patent to the view. She has simply transferred in a quite uncritical way the labored ideology of the "new Germany" to the study of an ancient problem. Greece was tottering to decay (democracy is mentioned as one of the destructive forces on pp. 4 and 5). There were foes without and foes within (the phrase, *innerer und äusserer Kampf*, is strikingly frequent; it is a pity for the author's thesis that neither Jews nor Marxist had yet become important). Hellas must renew her youth, and to this end thinker and doer must make an alliance; only so was salvation possible. It is everywhere clear that the author has read the past with an eye on the present and in so doing has created not a convincing and dependable historical treatise, but a highly colored and glowing epic lit with a "light that never was on sea or land," a kind of consecration and certainly a dream.

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*L'industrie du papyrus dans l'Égypte gréco-romaine.* By N. LEWIS. Paris: Librairie L. Rodstein, 1934. Pp. xiii+186.

In this useful book the author has collected and interpreted the available evidence concerning the growth, distribution, and uses of the papyrus plant. The arrangement of the material is good, and the presentation clear and pre-

cise. In those portions of the work where views may differ, e.g., in the meaning of *χάρτης*, *κόλλημα*, *σελῖς*, etc., and in the discussion of the extent of monopoly in Ptolemaic and in Roman times the method of exposition is satisfactory. Most of the material is, of course, not new, but papyrologists will be grateful to Mr. Lewis for his painstaking work.

Chapter i (pp. 1-22), which might have included a more adequate botanical description, is devoted to the growth and distribution of papyrus in Egypt, Africa, Syria, Babylonia, Etruria, India, Sicily, etc. In chapter ii (pp. 23-45) the author discusses in detail the various uses of the plant, exclusive of that as writing material. These include: food, baskets, mats, ropes, wicks, sieves, canoes, clothing, sandals, wood, cartonnage, wrapping paper, amulets, and even medicine. Chapter iii (pp. 46-93) deals with its use as writing material. Under Section A there is a discussion of the processes of manufacture, including a good interpretation of the *locus classicus* in Pliny *NH* xiii. 74, 77-82, and of the size, cost, and durability of rolls. Under B, probably the most valuable part of the book for the papyrologist, Mr. Lewis takes up the more controversial questions relating to the exact meaning of various terms: *χάρτης* (defined as a "blank roll"), *χαρτάριον* (a piece of papyrus containing writing), *χάρτιον* (often a synonym for *χάρτης*), *βιβλίον* (a written roll), *χαρτίδιον* (a small roll), *κόλλημα* (the pasted page of a roll), and *σελῖς* (a column of writing). Under C the leading grades of papyrus are listed and, in so far as the evidence permits, described. Section D presents a brief account of the expanding use of papyrus as writing material from the time of the Third Dynasty until its abandonment by the Papal Chancery in the middle of the eleventh century. Chapter iv (pp. 94-157) treats, under A, the cultivation of the plant in swamps and stagnant waters, the control exercised by the government to avoid overproduction, and the tax paid to the government; under B, the evidence for the location of factories, and the conditions of private and official sale. Under C there is a detailed and useful account of the tax known as *anabolicum chartae*; under D, a discussion of the *χαρτερά* (interpreted by the author as the tax imposed to establish the legal validity of a document); under E, the export of papyrus as writing material; and, finally, under F, a brief paragraph, with an instructive table, of the cost of papyrus. An Appendix (pp. 159-63) contains a discussion of the industry from the time of Diocletian to that of Justinian. Adequate indexes (pp. 165-86) conclude the book.

To the list of corrigenda considerable additions can easily be made, inasmuch as the text is marred by many errors. The circumstances under which the book was printed will explain some of them, but a mistake like *Vopsicius* throughout the text and even in the Index must be ascribed to the author. The interpretation given by Mr. Lewis of Plato *Apol.* 26 D should be rejected in favor of that presented by P. Collart, "Livres neufs ou vieux bouquins," in *Mélanges offerts à M. Octave Navarre* (Toulouse, 1935), pp. 95-99.

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